FINAL REPORT
A STUDY TO RECOMMEND NASA HIGH POWER LASER TECHNOLOGY AND RESEARCH PROGRAMS

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FINAL REPORT
A STUDY TO RECOMMEND NASA HIGH POWER LASER TECHNOLOGY AND RESEARCH PROGRAMS

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FOREWORD

This report has been prepared in conformance with Article IV, of NASA Contract No. NASW-2866, "A Study to Recommend NASA High Power Laser Technology and Research Programs". The Program objectives were:

- Review and analyze NASA HPL programs underway or proposed
    - Rank programs in order of importance
    - Recommend which programs should be:
        - Increased in size
        - Remain at same level
        - Discontinued
- Identify technology areas in which advances in the HPL state-of-the-art are required to satisfy mission requirements
- Compare test requirements for NASA missions with those of other missions to identify possible combined tests

An additional task was added in May 1976 to provide support for the NASA/OAST "Multi-purpose Space Power Platform (Theme 07) Study" in the following areas:

- Derive estimates of the requirements for laser-powered propulsion for orbit-raising
- Derive estimates of the requirements for laser-power transmission
- Derive rough cost estimates

FOREWORD (Cont'd)

The results of these efforts are contained within this report.

We wish to acknowledge the many useful discussions with both the NASA/OAST Program Manager, Joe Lundholm and the Director of the Research Division, F. Carl Schwenk. In particular, we have heeded their advice to be forthright and frank in our assessments at this risk of stirring up controversy. (We have tried to be very critical, but not caustic). We further wish to acknowledge the cooperation we received from the personnel at the various facilities:

Lewis Research Center - Don Connolley, Jack Slaby and their co-workers

Ames Research Center - Ken Billman and co-workers

Langley Research Center - Bob Hess and co-workers

Jet Propulsion Laboratory - Gary Russell and co-workers

Key contributors within W. J. Schafer Associates, Inc., were as follows:

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Program Manager

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CHAPTER I. MOTIVATIONS AND METHODS

A. INTRODUCTION

Late in 1975, W. J. Schafer Associates, Inc., (WJSA) was awarded a contract by the National Aeronautics and Space Administration to review and evaluate NASA's current exploratory efforts directed toward utilization of High Power Lasers (HPLs) and related technologies. The related technologies include such topics as laser energy conversion and laser photochemistry, all of which have been consolidated by NASA under the title, "Photonics". WJSA examined experimental and theoretical programs being supported by the NASA Office of Aeronautics and Space Technology (OAST) at the Lewis, Langley and Ames Research Centers, and at Jet Propulsion Laboratory. Particular attention was focussed upon the relationship between the work in progress at these laboratories and the long range applications which might benefit from continuance of that work.

Several different motives inspired NASA to undertake the study effort to be described in this document. In the first place, it was evident that a dispassionate overview of laser-related research within NASA could best be obtained from outside the space agency, simply because parochial views always develop within a bounded society. Additionally, it was clear that the overview should extend beyond NASA research to investigate possible connections with, or duplications of, work being pursued by other agencies of the government and by private industry. Thus, the success of the study depended upon broad knowledge on the part of the investigators of the entire gamut of laser research being pursued both in this country and elsewhere. It was necessary in addition, for the investigators to have full access to classified information and the ability to provide "sanitized" assessments of mutual relationships between NASA's laser programs and certain classified laser programs. Finally, NASA desired that the results of the study be disclosed in a frank and forthright manner so that dialogues and cross-fertilization of ideas could be inspired among the various NASA researchers and administrators. Consistant with the level of the study effort, the authors believe that all of these objectives have been addressed and satisfied.

B. EVALUATION CRITERIA

Five principle criteria have been employed in the evaluation process to be reported here. Of particular importance is the criterion of application identification: There are innumerable possibilities for interesting pure research in the rapidly growing science of laser physics; but, since NASA must operate with limited financial and personnel resources for laser research, it is necessary to inquire how these resources can be used most wisely to fulfill specific NASA needs for the future. The present level of laser-related NASA funding stands at approximately \$6 million per annum, compared with DoD high power laser funding almost two orders larger. (The distribution of funding and personnel among the various NASA facilities is shown in Table I-1). Since many details of the DoD efforts are obfuscated by classification, it is quite important for NASA to obtain sufficient information to permit concentration upon applications which will not involve duplications of research being pursued for similar DoD applications. In short, it is essential to identify direct potential benefits to specific NASA objectives that might accrue from ongoing NASA research efforts, while maintaining an awareness of related DoD research and development programs that will eventually become useful to the civilian space program. For example, for the *Laser Propulsion* application it is conceivable that much of the primary development program to produce the laser and the necessary adaptive optical system for projecting the beam, might best reside in DoD, while NASA might provide uniquely important contributions in the area of converting laser radiation to rocket thrust. Hence, the true NASA application would be *Laser-induced Thrust Generation* rather than the whole problem embraced by the *Laser Propulsion* concept.

A second evaluation criterion is uniqueness. Clearly, it is unlikely that a low budget research effort within NASA will produce significant advancement of laser science if it is competing directly with a very high budget effort within DoD. It is possible, however, that there exists a subset of the effort which is unique to NASA's needs and, therefore, is justifiable. For example, it does not make sense for NASA to attack the entire problem of the development of closed-cycle high power electric

TABLE I-1

LASER-RELATED NASA FUNDING

Facility	RTOP #'s	High Power Lasers $(F.Y. '76)	High Power Lasers Man Yrs.	Fundamental Photonics/ Quantum Electronics $(F.Y. '76)	Fundamental Photonics/ Quantum Electronics Man Yrs.	Institutional Management Support $(F.Y. '76)	Research & Program Management*
Ames	506-25-41	$ 200K	10	--	--	$ 116K	$ 409K
	506-25-32	--	--	$ 225K	10	187K	409K
Lewis	506-25-41	530K	30	--	--	290K	1.01M
	506-21-40	--	--	100K **	1	20K	34K
	506-22-41	--	--	30K †	6	90K	202K
Langley	506-25-43	150K	8.5	--	--	94K	302K
	506-25-31	--	--	320K	11.6	119K	412K
	506-21-42	--	--	50K **	0.6	11K	21K
J.P.L.	506-25-41	285K	5	--	--	(Included in R&D)	(Included in R&D)
	506-25-42	100K	2.5	--	--		
	506-25-31	--	--	750K	12.6		
NASA HQ.	506-25-41	100K	NA	--	--	NA	NA
	506-25-31	--	--	250K	NA	NA	NA

*Salaries, Vacation, Etc.

NA = Not Applicable

**Laser propulsion study under Power & Propulsion RTOP

†Plasma Dynamic Lasers RTOP

discharge lasers; but it can be shown that it is reasonable for NASA to investigate aspects of closed cycle EDL development which might lead to sustained operation at high power levels for long periods of time as required by both the laser propulsion and the power transmission concepts.

A third evaluation criterion is <u>relevance to other programs</u>, both within NASA and in the world at large. This is the inverse of the uniqueness criterion. Thus, for example, if NASA identifies some promising <u>new</u> concepts which might lead to high average power lasers for visible wavelengths, the work is probably justifyable because of its importance for many applications, including applications which may be equally as significant for other agencies such as DoD or ERDA as for NASA itself.

<u>Physical and technological feasibility</u> defines the fourth evaluation criterion. This may seem to be such an obvious measure of worth that one might ask whether the research would be proceeding at all if it did not seem feasible. The fact is, however, that, in any avant garde field of science, fatal flaws may remain concealed for an extended period of time. Costly errors can be avoided only by continuous peer review of both the basic physics and the experimental approach underlying each project. Careful scrutiny such as this has permitted us to recognize in the present study certain unrewarding efforts where, for example, an interesting concept and a good experiment could only lead to a very inefficient laser.

The fifth and final evaluation criterion questioned the <u>overall significance</u> of each research effort. Again, this type of inquiry may smack of the jejune--like questioning the value of motherhood! The authors of this study feel, on the contrary, that one of the most important questions that can be asked about any program involving considerable dedication of time and money is, "Why are you doing this?" Indeed, such questioning has led us to conclude that some of the larger efforts are of dubious value, while some of the smaller "sleepers" may have considerable significance in the long run.

C. PROCEDURE

Before the above evaluation criteria could be applied, it was necessary to organize a comprehensive but succinct outline of all of the work being performed at the NASA centers. For this purpose much data was supplied by NASA, including written program descriptions prepared under the supervision of the group leaders at each center. Further details of the work were obtained through personal visits by WJSA staff members to each of the laboratories. Where necessary, certain annotations were added to the outline, in some cases to call attention to debatable scientific issues, and in other cases to quote opinions from the NASA program descriptions which might be either debatable or simply informational. Examples of the resulting worksheets are shown in Appendix A.

To proceed with the evaluations, copies of the outlines were supplied to four laser and technology planning experts at WJSA, together with a letter explaining the evaluation criteria. It was requested that each of these experts (who jointly possess over fifty man-years of experience with lasers and other closely related technology) should review every item in every outline and referee it for both its intrinsic value and its scientific and technological promise. These evaluations were then merged together to form a coherent criticism of the entire NASA program.

An interesting aspect of the refereeing process was the strong agreement of the evaluations, which were generated by each person in complete independence. This greatly simplified the merger of the results. It also made it possible to easily identify points of disagreement, which were then resolved by negotiation among the referees to obtain a common opinion.

The results of the preliminary evaluation were assembled into a detailed briefing which was presented to representatives of NASA headquarters and each of the four NASA research laboratories at a Program Review meeting held at Lewis Research Center in April, 1976. This provided an open forum for criticisms of the work and feedback concerning the conclusions. The results were quite gratifying: Although some NASA efforts were heavily criticized by WJSA and some of these criticisms were strongly defended by NASA, there was near unanimity on the point that the study had provoked a healthy reassessment of the NASA programs and a new degree of cross-fertilization of ideas and opinions among the participants.

After the April meeting, written critiques of our evaluations were obtained from several NASA sources, and Lewis, Ames and JPL were re-visited to insure that all viewpoints were adequately understood before the writing of this final report of the study was undertaken. Several program changes had already been implemented at some of the centers, and these are noted as appropriate in the material which follows.

D. DOCUMENTATION

Most of the remainder of this report constitutes a compact review of our evaluation of each NASA program. Chapters II, III, IV, and V respectively discuss the work at Ames, Langley, Lewis, and JPL. Chapter VI gives a brief description of certain ongoing projects in other agencies which have particular relevance to NASA interests. Chapter VI also discusses some new concepts now being considered by other agencies which could be of great significance to NASA. Chapter VII concludes the report with some observations and conclusions reflecting our views concerning the past and future of NASA's High Power Laser and Photonics programs. Throughout the report, suggestions will be made concerning continuance or discontinuance of certain programs, with an eye toward new applications.

CHAPTER II. RESEARCH AT AMES RESEARCH CENTER

A. INTRODUCTION

Laser related research at the Ames Research Center is carried out by two groups administered under the Physical Gas Dynamics and Lasers Branch. The groups and their Research and Technology Operating Plan numbers are, respectively, High Power Lasers, RTOP 506-25-41, and Quantum Electronics, RTOP 506-25-32. The former is oriented more toward experimental hardware and developmental programs while the latter addresses fundamental research questions which, although "high risk", may have large long-term payoffs. We shall divide our commentary along lines naturally resulting from this division of work.

At the outset we take note of the fact that Ames is perhaps the most academically oriented of the four NASA Institutions being discussed in this report. An unusually large number of technical papers originate at Ames. Indeed, this is a very creative laboratory and some of the work is quite *avant garde*. The authors of the present study feel, however, that there has been some tendency toward myopia within the Ames programs. This has led to the growth of certain experimental and theoretical research efforts, which, while always being intrinsically interesting, reflect inattention to important work which has been done at other laboratories and also insufficient consideration of the ultimate usefulness of the endeavors. A stated primary goal at Ames is new ideas, but as we will show later, some of the work is not really new. Moreover, lack of focus upon applications had led to continuation of certain efforts which could only lead to inefficient end-products that would be of little ultimate use to NASA. These will be frankly discussed in the following pages.

The authors hasten to add that most of the criticisms made by WJSA have already been acted upon by the Ames program managers during the course of the study. Thus, most of the discussion which follows can now be regarded as hindsight. The prognosis for ongoing programs is very favorable.

B. HIGH POWER LASER GROUP

Work within the High Power Laser Group is divided among four principal areas of concentration, namely, gas dynamic lasers, carbon monoxide electric discharge supersonic lasers (COEDS), laser energy conversion (LEC), and related theory. These will now be discussed in turn.

1. Gas Dynamic Lasers

At the time that we began our study, the gas dynamic laser program included both an arc-heated CO_2 gas dynamic laser experiment and supporting computer models. These activities are now being phased out, both in response to findings of the experimental program itself and to conclusions resulting from our studies. The primary contribution of WJSA consisted of an analytical demonstration of the fact that the mass flow efficiency of the laser was intrinsically too low for it to be competitive with other available options. This work* is included

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*W. J. Schafer Associates Technical Memo 76-01, "A Review of the NASA/Ames Arc-Heated Gas Dynamic Laser Performance", G. W. Zeiders, January 14, 1976.

in this report as Appendix B. In principle, this conclusion could have been reached much earlier, thus saving a large amount of dedicated effort. This fact is illustrative of the utility of a line of inquiry which we feel should be applied early in any new program. It will be reviewed here as a "post-mortem" for the Arc-Heated CO_2 GDL.

One can begin by simply asking the question, "What are NASA's intended applications for High Powered Lasers"? At the present time the only clearly identified applications are *Energy Transmission* and *Propulsion*. Whether or not these HPL applications are viable depends upon several key issues. Of primary importance is the question of ultimate efficiency of the entire postulated system. Since the laser itself and its associated optics would represent a major fraction of the needed system, their efficiency must be a central concern, regardless of whether the application requires the laser to be in space or on the Earth's surface.

Efficiency is an issue which encompasses both economics and physical practicality. Since the Ames Arc-Heated GDL device used CO_2 as a lasing medium, it was tied inextricably to several fundamental sources of inefficiency. First, the laser and pointer-tracker would require larger optics than a shorter wavelength laser. This, of course implies more weight, either in orbit or on the ground (or both) and, therefore, more expense and more deployment complexity. Additionally, the physics of the CO_2 laser limits the ultimate quantum efficiency to 41%. In fact, typical input-to-output power efficiencies for CO_2 lasers are far less than 10%, and the fuel mass-flow efficiencies vary greatly with operating conditions.

Still another source of inefficiency is encountered if a CO_2 laser beam must propagate through the atmosphere. Both aerosol scattering from vapors and particles, and absorption by atmospheric CO_2 pose serious loss problems which can only be partially overcome in the best circumstances by using a variety of methods such as short pulses, isotopic detuning, etc.

Taken together, all of these difficulties militate strongly against the likelihood that the CO_2 Arc-Heated laser could survive a careful, application-oriented system optimization competition. This program had little focus on applications, and, although the computer predictions and verifications of experimental results were well done, there was little real justification for the entire effort. DoD and private industry's past experience with GDLs indicated these results nearly a decade ago, and NASA could have avoided relearning the same lessons if better rapport had been established with existing experience elsewhere. We shall return to this point later when we recommend an expanded budget for interlaboratory travel and better coordination with other agencies.

We conclude this section with two final caveats. First, there remains always the possibility that laser efficiency is not the key point. For example, cost per installed kilowatt may be a more important criterion in a *Power Transmission* application scenario; and, for *Propulsion*, Billman at Ames has pointed out that the beam may communicate energy to the propellant gas by inverse bremsstrahlung more readily at even longer wavelengths than 10.6 microns. Secondly, CO_2 may have some advantages for space-only closed-loop applications because of its relatively benign operating characteristics and lack of a requirement for auxiliary systems such as power supplies. The only way such possibilities can be justified or laid to rest, however, is to first define the application and then to apply systems analysis to evaluate the options. Only after this has been done should research and development programs be initiated, provided, of course, that sufficiently promising technological concepts are identified.

2. Carbon Monoxide Electric Discharge Supersonic Laser (COEDS)

With regard to the CO Supersonic EDL program at Ames, there is some "good news" and some "bad news". The bad news is that, with DoD funding, Northrop and Boeing have done many things at a level with which NASA can't compete. For example, Northrop has constructed one of the most powerful lasers in "Christendom", a supersonic blowdown CO EDL. Moreover, Northrop has done much of the programming necessary to characterize multi-line CO laser propagation in the atmosphere.

It is interesting to note that some of the most graphic data illustrating the superiority of CO over CO_2 originated at NASA Langley Research Center several years ago. These data, which need to be updated to incorporate recent more precise AFCRL data, are shown in Figures II-1 & 2. The atmospheric propagation advantages of CO over CO_2 are evident, especially when the laser is located at suitable mountain-top locations having low aerosol content. The CO line chosen does not even represent the propagation of the better CO lines! Even so, the CO_2 is at a considerable disadvantage because it continues to be absorbed

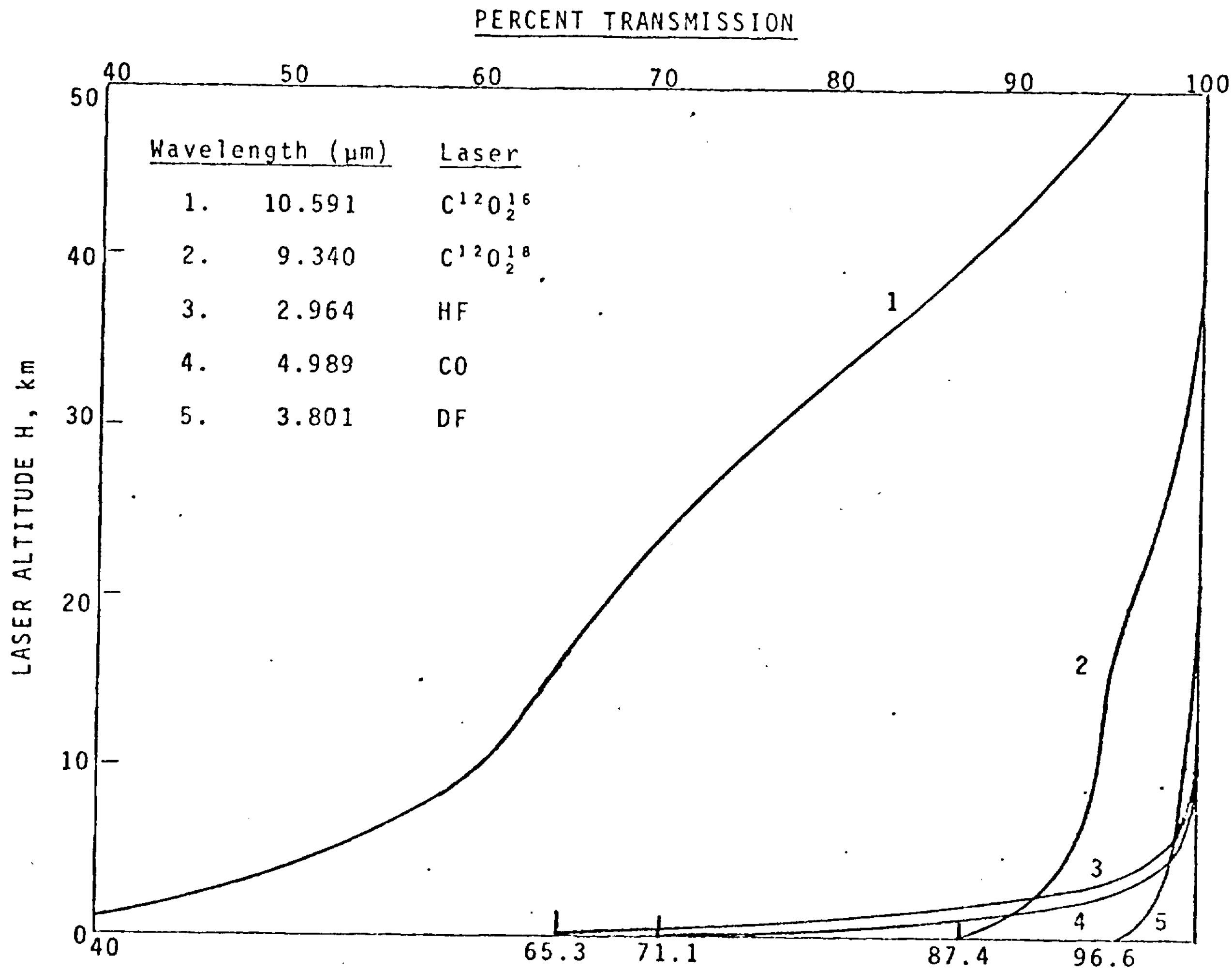

Figure II-1: Vertical Transmittance for Laser at Altitude H Propagating to Space for Select Laser Wavelengths (1966, 45°N July Model Atmosphere)*

* From work performed at NASA Langley Research Center

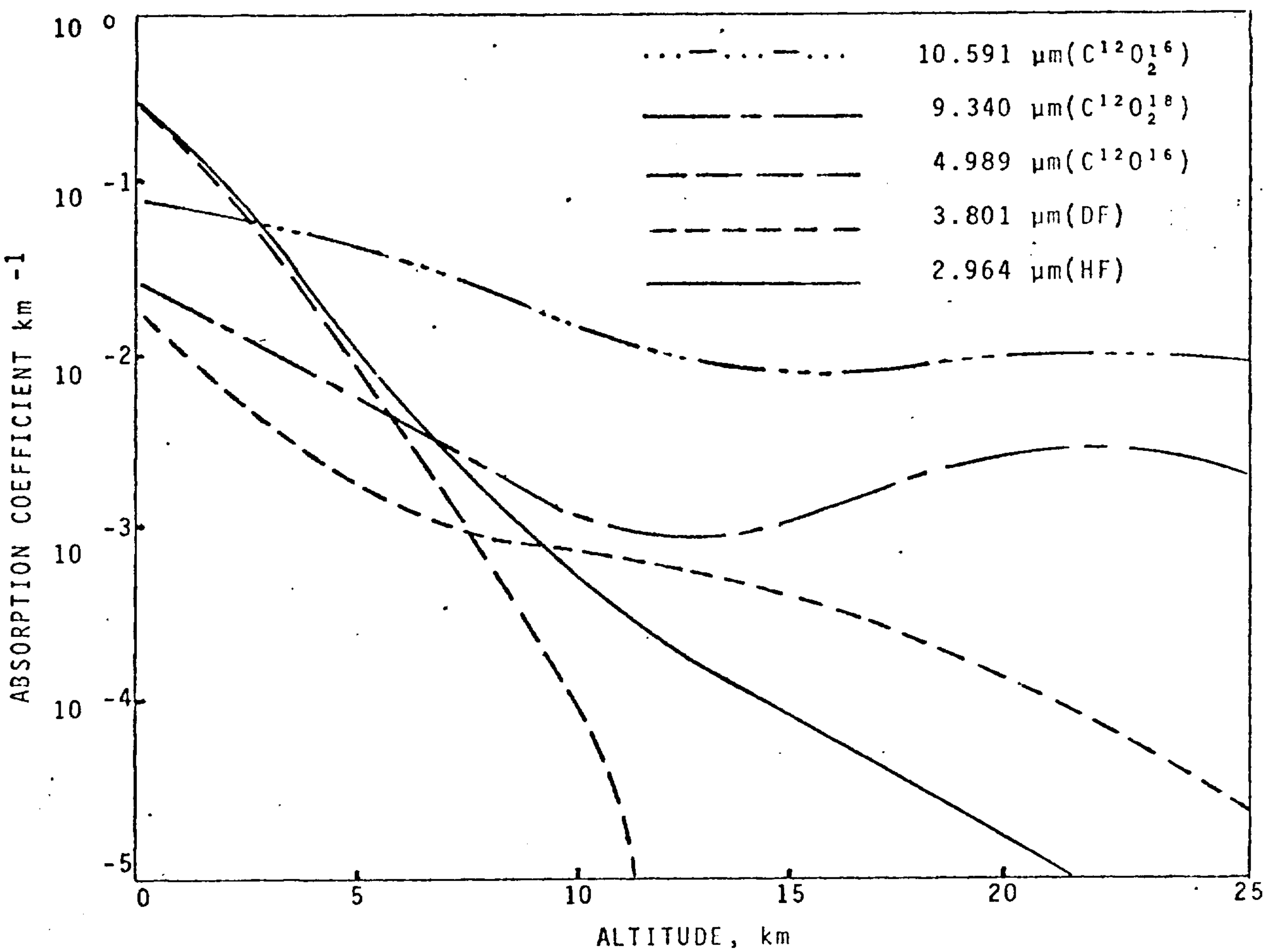

Figure II-2: Variation Of Calculated Absorption Coefficient With Altitude For Selected Laser Wavelengths (1966 45°N July Model Atmosphere)*

* From work performed at NASA Langley Research Center

by atmospheric CO_2 mixed to high altitudes.

In the "good news" category, the Ames program to investigate alternate ionization schemes has singular signifacance. To our knowledge DoD is not investigating such techniques, although the Air Force Weapons Laboratory and other agencies are extremely interested in the concept. If a scalable, efficient CO laser using no electron beam can be developed, all users will benefit greatly because of the elimination of the weight and complexity associated with the beam source and its power supply. Scalability of the non-E-beam laser is the key issue which must be resolved, however. At present new E-beam CO lasers seem to be only about half as efficient (20% overall) as predicted (40%).* There is a clear need for more work on kinetics to establish the reason for this discrepancy.

Other aspects of the COEDS project are very appealing, such as its potential for high efficiency, closed cycle, non-cryogenic operation at high power levels. In short, this is a project that must proceed in good coordination with other laboratories. The concept is clearly useful for applications both within NASA and in other agencies such as DoD. The desirability of shorter wavelengths and high efficiency are readily apparent.

In the wake of the present study, Ames has initiated several activities to keep abreast (or anticipate) CO developments in the DoD and ERDA sectors. Arrangements have been made for Ames to receive all AFWL CO report distributions, in addition to the

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*Potential CO efficiencies as high as 70-90% have been predicted by D. J. Monson in NASA Technical Memorandum # TM x-62, 438.

material routinely received from ARPA. Also, Ames personnel have recently visited or plan to visit Northrop, Aerospace, Hughes, Stanford Research Institute, and Boeing to discuss short wavelength laser research and to view existing programs.

3. Laser Energy Conversion

In this section the discussion shifts to methods of utilizing high power laser radiation rather than dwelling upon the ubiquitous problem of how best to generate it. In addition to Ames, Lewis, Langley and JPL have programs related to this problem. All of the Ames efforts are directed toward direct conversion of laser energy into electricity, shaft horsepower, or storable chemical energy without the necessity for an inefficient conversion step such as heating water to make steam.

Ames laboratory is a distinct leader in this important research, not just within NASA, but nationally. Neither ERDA nor DoD has similar large programs, although M.I.T. has several studies underway. The potential payoffs for all energy users are so consequential if success is achieved in any of the areas of interest that this must be regarded as a program of major significance for future applications. In fact, the authors feel that NASA might do well to concentrate more talent and effort into this area. As implied in Chapter I, here is an example of a "sleeper"--a program which is modestly funded and not widely recognized, but is nevertheless filled with promise. It rates near the top when tested against the five evaluation criteria.

Particulars of the laser energy conversion work are available in the proceedings of two conferences organized and reported on by Ames*. Much of the detailed research work is contracted out by Ames, as shown in Table II-1. The current status is given in Table II-2. Additional relevant work is being performed in the Quantum Electronics group. This will be discussed below under the headings of Harmonic Conversion (to shorter wavelengths) and Laser Isotope Separation.

As to criticisms and caveats, one of our most important concerns is that the definition of conversion efficiency cannot be restricted to the receiving device alone. For example, the possible use of laser photocatalysis to separate water into storable hydrogen and oxygen could be of very great importance. But one must also know the energy input to the laser itself in order to determine whether the total separation efficiency is greater than direct electrolysis. Obviously, the most efficient energy converters should be matched with the most efficient lasers. Also, hidden bonuses might be available. For instance, photocatalytic hydrogen separation from water might conceivably be better accomplished in a hybrid system using focussed solar energy augmented by laser radiation. The overall efficiency might be much higher than in the direct laser separation case.

Another concern is that, while there are doubtless many applications for laser energy converters, many applications remain unidentified. This could lead to situations where the research and development work carries on to the point of success and is then "put on the shelf" to wait for a mission. Because of such

*Proceedings of the First and Second NASA Conference on Laser Energy Conversion held at the NASA Ames Research Center, Moffett Field, California. (See NASA SP-395)

Table II-1

AMES RESEARCH CENTER

LASER ENERGY CONVERSION PROGRAM*

APPROACH	DEVICE	OUTPUT	INVESTIGATOR	FUNDING
Thermodynamic Engine	General Study of Laser-Driven Engines	Mechanical or Electrical	M. Garbuny, Westinghouse Research	18 K
Thermodynamic Engine	Free-piston Stirling Engine	Electricity	G.Lee, NASA-Ames, W.Martini, Univ. Washington(consultant)	16 K
Thermionic Diode	TELEC	Electricity	N.Rasor, Rasor Associates	40 K
Optical Diode	3-D DIODE ARRAY	Electricity	T. Gustafson, Univ. Cal. Berkeley	40 K
Laser Assisted Chemistry	Photo-Catalytic Dissociation H_2O	Hydrogen, Oxygen	W. Bottoms, Princeton Univ.	40 K
Conversion of High Intensity IR to Shorter Wavelengths	Molecular Gas Frequency Upconverter	$\frac{\lambda \text{Laser}}{n}$	C.Y.She, Colorado State U.	10 K

*From a presentation of Kenneth Billman at the NASA OAST Program Review held at Lewis Research Center, April 21, 1976.

Table II-2

CURRENT STATUS OF

LASER ENERGY CONVERTERS*

DEVICE	EFFICIENCY(%) Present	Future	ACCOMPLISHMENT TO DATE	TECHNOLOGY STATE OF ART	DEMONSTRATED EXPERIMENTALLY	BEST POSSIBLE RESULTS (Increased Funding)	WINDOW REQUIRED
GENERAL ENGINES	--	75	High Theoretical Conversion Efficiency	YES	NO	100 KW Engine Built	YES
STIRLING ENGINE	50	75	Engine Driven by CO_2 Laser	YES	YES	100 KW Engine Built	YES
TELEC	--	45	Conceptual Model Made	NO	NO	Pre-Prototype TELEC Built	YES
3-D JUNCTION ARRAY	--	--	Single Junction Can Be Made	NO	NO	Array Fabricated	NO
PHOTO-CATALYTIC DISSOCIATION H_2O	--	30	Conceptual Model Made	NO	NO	Dissociation of H_2O Demonstrated	YES
MOLECULAR GAS UPCONVERTER	--	30	Conceptual Model Made	YES	NO	Efficient Up-Conversion Demonstration	YES

*From a presentation of Kenneth Billman at NASA OAST Program Review, Lewis Research Center, April 21, 1976.

possibilities, it is particularly essential that the cross-fertilization process between agencies, centers, and industry be enhanced. All too often, people at the same laboratory remain ignorant of each other's results and needs! It is incumbent upon project leaders to keep abreast of interdisciplinary tie points.--Joseph Lundholm of NASA headquarters has pointed out that JPL and Goddard Spaceflight Center have most of the missions. Hence, the future of energy conversion work at other centers may depend upon getting their attention.

The two technological issues touched upon in this work which we believe to be of widest significance are the practicality of laser dissociation of water and the efficiency questions associated with possible laser up-conversion from IR to visible wavelengths. NASA is already moving to institute new RTOPS in these areas. One, dealing with Photo Enhanced Chemistry will include solar pumping. A modification of the Quantum Electronics RTOP will spur further development of short wavelength lasers and IR up-conversion.

Since other NASA centers are now expressing interest in carrying Ames device concepts into the experimental stage, it is important that the work progress in a well coordinated and fully justified fashion.

4. Theory of Vibrational Energy Transfer from Diatomic Molecules

Much new information on energy transfer is still needed by all laser researchers. It is very likely that the Ames theoretical group could work together with other agencies such as DoD in numerous areas. We recommend that immediate attempts be made to coordinate with AFWL and AFCRL, for example. An ongoing, in-depth check is needed to avoid duplications of effort. Since ERDA's interest in lasers is now growing, overtures should be made there also.

Good general physics programs such as this one are almost always worthwhile. Kinetics and energy exchange studies are the things which lead to new and more efficient lasers. For example, the original GDL evolved from air properties studies for the reentry physics program. HF and DF chemical lasers evolved from the HF rocket program.

C. QUANTUM ELECTRONICS GROUP

We turn now to consideration of programs of the Ames Quantum Electronics group. Only brief comments will be given concerning the smaller or less promising efforts.

1. Electronic Recombination Laser

The Electronic Recombination Laser represents an attempt to make a gas dynamic laser for wavelengths shorter than 2.5 μ. The idea is to produce a population inversion in lower electronic states of an ionically recombining atomic vapor by collisional quenching with admixed molecules. This is surely an interesting physical concept; and any new process which holds promise for producing a high power, short wavelength laser is definitely worth investigating. Nevertheless, a laser must be both efficient and scalable to be justifiable. We are of the opinion that this concept will probably produce a working laser, but that the efficiency will be low. The key uncertainty, as the researchers at Ames know, is the magnitude of the quenching cross-sections. It seems reasonable to complete the theoretical modelling and efficiency estimates now in progress. The work should be terminated, however, if the results point to a low-payoff end-product which will not contribute to NASA's applications needs. (We understand that Ames now plans to curtail this effort within the next few months).

2. <u>High Brightness Laser Facility</u>

This program consists of a pure research exercise directed toward the achievement of laser action in the soft x-ray region. The idea is interesting, but does not seem to be relevant to any NASA application within the foreseeable future. Moreover, the terrawatt laser required would probably consume much effort and money with little probably return for NASA. Thus, there is little justification for continuing the program. Accordingly, Ames has decided to terminate the work.

3. <u>Laser Isotope Separation and Photochemistry</u>

In addition to the work previously discussed under the heading of Laser Energy Conversion, Ames is working on more esoteric aspects of photochemistry. (Indeed, selective excitation of isotopic species in molecules still qualifies as photochemistry!) The trouble is that there seems to be no NASA mission requiring isotope separation *per se*. Perhaps in the far future there might be applications such as scavenging tritium from a nuclear fusion power plant or rocket engine, but this doesn't seem to be a viable concern for the present. In any event, this topic seems to fall clearly within the province of ERDA and not NASA.--This in no way reflects any disfavor upon the part of the work at Ames which is concerned with laser induced chemistry, e.g., for the efficient dissociation of water into hydrogen and oxygen. That work should certainly be expanded. Ames is now attempting to improve its communication with ERDA. It is very important to identify more applications for laser enhanced chemistry in the near term.

4. Harmonic Conversion

The concept of up-converting a high power infrared laser so that the second or third harmonic falls in the visible or near-visible regime is potentially very interesting. The big question is whether it can be proven efficient. Both Rockwell and United Technology Corporation have observed that third harmonic radiation occurs naturally in HF/DF chemical lasers, but the conversion efficiency is quite small. This may have nothing to do with the conversion method under study at Ames, however. Ames is attempting to use vibrational-electronic transitions having large matrix elements to achieve high efficiency. Theoretical calculations have thusfar been very encouraging.

For spaceborne lasers, another element enters the efficiency discussion, namely the weight savings associated with smaller optics. One can assume a scaling law with

$$\text{Weight} \propto D^{2.5},$$

where D is the diameter of the primary transmitter aperture. Of course, $D \propto \lambda$, so the savings should be considerable at short wavelengths. Hence, it is possible that only $\sim$ 10% conversion efficiency might still lead to an advantageous system. Such systems analysis should be done before this project proceeds very far.

It is interesting to recall that the development of harmonic up-conversion played a central role in the development of radio technology. Perhaps history will repeat. In any event, this is an exciting concept that should be vigorously supported at the present time.

5. <u>Theoretical Studies in the Quantum Electronics Group</u>

As in the case of the High Power Laser theoretical support, there is much good and justifyable work being done here. The work supports various aspects of the projects discussed in sections II-C.1 through II-C.4 above. It is clearly relevant to the search for new lasers and to guiding the development of existing lasers. As always, it should be borne in mind that the main thrust should be directed by the applications. These are, at present, *Laser Propulsion*, *Power Transmission*, and *Laser-Induced Photo-chemistry*. The appurtenant key issues are *Scalability to very High Power Levels*, and *Wavelength Sensitive Considerations* (such as *Size and Weight of Optics* and *Opacity of Gases and Plasmas*). Again, we note the "systems flavor" of some of these issues. Often theoretical research groups are not systems oriented; and, therefore, they should be alerted to the fact that systems support may be needed to augment and justify their efforts.

Caution is also advised regarding other "bear traps" which are more technological than physical. For example, in photochemistry (and isotope separation), *Peak Intensity* may be a more important issue than *Scalability*.

6. Tunable Laser Laboratory

The final division of work in the Quantum Electronics group encompasses a versatile laboratory effort which is dedicated to accurate experimental determination of cross sections, rate constants, oscillator strengths, etc., which are of interest for the various types of lasers being studied by Ames. The program seems to cover the needs of the other Ames research efforts comprehensively. It is, for the most part, application oriented. Hence, the work seems eminently justifiable. The program appears to be so well motivated that we have recommended its expansion, which, we understand, has already been initiated.

D. SUMMARY OF AMES FINDINGS

Our findings and recommendations reflecting the foregoing discussions are summarized in Table II-3. As previously indicated, most of the suggested changes have already been implemented by Ames. Our overall impression of the Ames laser program is quite favorable. The Physical Gas Dynamics and Laser Branch possesses an energetic and creative staff, and the prognosis for future results is excellent.

Table II-3

EVALUATION OF NASA AMES PHYSICAL GAS DYNAMICS AND LASERS BRANCH

	RESEARCH PROGRAMS / EVALUATION CRITERIA	Application Identification	Uniqueness	Relevance to Other Programs	Physical and Technological Feasibility	Overall Significance for NASA	Overall Mean Value	W. J. Schafer Recommendations
HIGH POWER LASERS GROUP	Arc-Heated Gas Dynamic Lasers	1	2	1	2	0	1.2	T
	COEDS Laser	3	2	3	3	3	2.8	S
	Laser Energy Conversion	3	3	3	3?	3	3.0	E
	Theory of Vibrational Energy Transfer	3	3	3	3	3	3.0	S
QUANTUM ELECTRONICS GROUP	Electronic Recombination Laser	3	3	3	1	1	2.2	T
	High Brightness Laser Facility	0	2	0	1	0	0.6	T
	Isotope Separation	0	1	2	3	0	1.2	T
	Photochemistry	3	3	3	2	3	2.8	E
	Harmonic Conversion	3	3	3	2?	3	2.8	E
	Theoretical Support	3	3	3	3	3	3.0	S
	Tunable Laser Lab	3	3	3	3	3	3.0	E
	OVERALL MEAN SCORE	2.3	2.6	2.5	2.4	2.0	2.3	

Significance Levels:
0 = None
1 = Small
2 = Fair
3 = High

Recommendations:
T = Terminate the program
S = Sustain at present level
E = Expand the program

CHAPTER III. RESEARCH AT LANGLEY RESEARCH CENTER

A. INTRODUCTION

The Langley Laser and Molecular Physics Branch has been involved in research on high power laser concepts, propagation studies, and fundamental "photonics". (Refer to Table I-1.) During the course of the present study the work has been re-directed to phase out the laser and propagation efforts and focus on photonics. The emphasis now resides in areas related to (1) photoconversion of energy from broadband sources to laser radiation, (2) exploratory studies of solar and nuclear energy sources to drive lasers directly, and (3) photochemistry, including hydrogen production from water and photochemical cells for the production of electricity. For the purposes of the present report, however, we shall adopt a historical posture and shall discuss our findings concerning the program as originally structured. As in the case of our discussion of all of the other laboratories, we shall endeavor to reflect not only our own criticisms, but also the rebuttals and clarifications provided by Langley since our midterm progress report.

By way of general criticism, we observe that Langley has some very talented people with very creative ideas; but there is also a lack of coherence in the overall set of activities which gives an impression of "shotgunning around". In several instances, it was difficult for us to distinguish the level of effort and state of progress of the work. In other words, when we examined a particular line of investigation, there was a problem in making distinctions among questions such as:

(a) Is this concept simply an interesting idea or has it been physically justified and substantiated by analysis?

(b) Has a significant experimental effort or computer simulation been mounted to test the concept? What is being done?

(c) Has the experiment (or model) progressed to the point of producing useful data, or is it still being formed and adjusted?

(d) What is the actual level of effort and time scale?

(e) How do the creative inputs and work efforts divide between the resident staff at Langley and outside contractors such as Javan at M.I.T. and Cool at Cornell?

Some of these difficulties are reflected in our Langley working outline contained in Appendix A. It is our opinion that most, if not all of these uncertainties could be resolved by better attention to communication and documentation at Langley. It is not sufficient only to point to publications ultimately resulting from the work, because this short-circuits the processes of routine progress assessment and peer review during the critical early stages of the work. The data supplied to us by Langley (position papers, etc.) differed from that of the other NASA labs in that there was a noticeable lack of conciseness.

Brief, objective Quarterly Progress Reports <u>do</u> have value if thoughtfully written and adequately circulated*

As to the research programs themselves, Langley's efforts were apportioned in three areas, namely (1) High Pressure CO_2 Tunable Lasers, (2) Atmospheric Transmission Studies, and (3) High Energy Molecular Lasers. These will now be discussed.

B. RESEARCH PROGRAMS

1. High Pressure CO_2 Tunable Lasers

The kernel of this idea, which originated at Langley, is that pressure broadening in a high pressure laser cavity permits the laser to be tuned over a limited range. In the case of CO_2, the broadening amounts to $\sim$ 3 GHz per atmosphere of pressure, allowing the laser to be tuned far enough off atmospheric absorption lines to considerably improve the propagation. In particular, at altitudes above 2.5 Km, Langley finds that the transmission will be improved from 50% to 90%. Langley also suggests that the same principle can be applied to other types of lasers at other wavelengths. Of course, the question remains as to whether improved propagation is needed at other wavelengths (refer again to Figures II-1 & 2), and whether the tuning range provided by high pressure would permit an improvement, if desirable.

It is difficult to dissect this concept because it simultaneously embraces a large subset of other concepts and problems.

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*The criticisms in this paragraph have been added in the final report and have not been rebutted by Langley.

Taken by itself, the central idea of using pressure broadening to attain tunability is quite interesting. Among known high power laser propagation problems, the idea clearly has most significance for CO_2, because CO_2 laser lines are directly matched with absorption by CO_2 itself in the atmosphere. But in Chapter II-B.1 we have already discussed the several strong objections to an application-oriented system built around a CO_2 laser. Indeed this is why DoD is rapidly shifting to short wavelengths for all applications being seriously contemplated. So the question again arises: If there is no proven application for the laser or the concept, why pursue the project? Prove the need first!

Langley maintains that the main purpose of the effort was to establish proof of a new concept in atmospheric laser transmission. This appears to have been accomplished by computer modelling. But, then, Langley sought to substantiate the concept experimentally. A CO_2 laser (built by Javan under contract) was chosen for the proof test, "because it was further developed". Still, at the time the effort was terminated, the data in hand were very crude. Better wavelength resolution and line identification was needed to accurately quantify the pressure broadening and tuning. Even if this had been accomplished, however, it is clear that there were several other problems waiting in the wings which would have required major research efforts before the high pressure laser concept could have been proven viable:

Two of the key issues in this remaining subset are the needs for frequency stabilization and better beam quality. For new missions proposed by Langley, such as "chirped radar" (for "photon missions" to planets and satellites), frequency stability on the order of $\pm$ 1 MHz is needed, but only $\pm$ 100 MHz had been attained by Javan in the high pressure device (by the Ring Method of stabilization). Meanwhile, M.I.T. has achieved stabilities to better than $\pm$ 1 Hertz in other types of lasers.

Beam quality is traditionally poor in high pressure laser cavities. DoD is attempting to improve beam quality and propagation with advanced adaptive optics schemes. Langley proposed another new idea for improving beam quality in the high pressure laser which may, in fact, point to the most valuable subset of the entire concept. The idea is to make a highly uniform, large volume lasing region by resorting to new methods of volume preionization. In particular, Langley proposed the admixture into the lasing medium of an organic "seed" gas, having very low ionization potential. This is a very interesting proposal, because it is even possible that the laser itself might sustain the discharge in such a mixture; and it is also possible that this approach might be useful in many other types of lasers such as excimers, for example. Other volume preionization schemes mentioned by Langley, such as UV flashlamps and nuclear particles, are much less exciting to contemplate because they would require large amounts of external paraphernalia, while organic gases conceivably might be self-sufficient. Nevertheless, volume preionization is very important, and promising ideas are worth pursuing. This is especially true for visible lasers which, thus far, depend almost entirely upon electric discharge stimulation.

It is our belief that this research should be sustained alone, decoupled from the high-pressure CO_2 laser concept, which seems to have little in-depth justification.

Finally, a few remarks should be made concerning Langley's support of Javan's research. Javan has made many major contributions in the laser field and should definitely be supported. Unfortunately for NASA, however, the list of research topics listed by Langley for Javan's contracts is so long that it is non-specific by *fiat*. (See our Langley outline in Appendix A., I-C.1 & 2). This is a classic example of a situation where NASA could narrow the scope through a careful assessment of applications and benefit greatly in the process. The volume preionization idea would be a likely topic upon which to focus attention.

2. Atmospheric Transmission Studies

Langley Research Center has an appreciable history of successful research in areas related to atmospheric spectral properties. Numerous contributions to the literature on opacity, transmission, and molecular composition as functions of wavelength, altitude and climatic condition have been made by various groups at this laboratory. Hence, there is a precedent for this sort of work at Langley. The problem which had to be confronted in the present study was whether adequate justification exists for the atmospheric propagation studies that were being attempted by the Laser and Molecular Physics Branch. But first it was necessary to try to distinguish what had actually been accomplished from what Langley was trying to do or thought it might do.

The stated intent of the Langley work was to augment data inputs to existing NASA and DoD propagation computer codes to improve the accuracy. High resolution spectrographic studies were to be conducted in two modes: (1) Long-path laboratory measurements using tunable diode lasers, and (2) High-resolution atmospheric line profile scans using a tunable laser-heterodyne spectrometer to observe the sun.

Of course, there is always a need for more refined atmospheric data. Understanding the basic physics of the Earth's atmosphere and its interaction with radiation at all wavelengths is an on-going project that will be with us for many years. At whatever wavelengths lasers may be required to propagate, it will be very necessary to understand the absorption line structure (and its temporal variations) in great detail. Moreover, since it is quite likely that different agencies having different requirements will use lasers at different wavelengths, various parts of the spectrum will be of interest to numerous different agencies. For example, there appears to be a strong predjudice in ARPA and the Navy toward the use of HF/DF lasers because of the short wave-length and high efficiency. NASA, on the other hand, may discover that CO lasers are the best expedient for near-term long duration operation at high power levels. But, then, it would seem that before any effort or money is expended by NASA to study the fine details of propagation the best candidate spectral regime should be chosen. Does it make any sense for NASA, with its very limited laser resource dollar, to support a significantly larger DoD program without DoD support, as would be the case if Langley carried out its proposed program of surveying the 2.7-3.7 micron regime?

Now, if we turn to the actual Langley effort, what do we find? Many possible measurements are mentioned in the position paper and its addenda, but very few measurements have actually been completed, and they have little or no relevance to any NASA laser applications. Perhaps the nearest thing to a useful proposal would be a survey of the 9.2-9.6μ region to determine ozone absorption, because this is potentially significant for the newly exploited 9.28μ CO_2 transitions being investigated by AFWL. But, again we think that NASA should seek AFWL support before embarking upon this effort.

The only measurements which appear to have been completed (in the solar mode) were done jointly with Airborne Instrument Laboratory, using a $C^{13}O_2^{16}$ laser local oscillator for absorption measurements of NH_3. (Ammonia is not a major component of the Earth's atmosphere). The developmental effort to produce a tunable laser local oscillator for a tunable heterodyne receiver never succeeded, partly because of a need for single-mode diode lasers.

In the balance, we feel that it is a good thing that this program was curtailed. The return from the effort was low, and the justifications seem to be lacking. The hands-on experience with heterodyne receivers may have some value for future decision making.

3. <u>High Energy Molecular Lasers</u>

The third category of laser related research work at Langley includes several high energy molecular laser efforts. Much of the work has been done in collaboration with Professor Cool at Cornell University.

One effort has been directed toward understanding chemical laser instabilities. Cool has concentrated on diagnostics of vibrational-rotational (V-R) relaxation and some aspects of rotational-rotational (R-R) relaxation. (This work differs slightly from that of J. J. Hinchen at United Technology Corporation, who has been primarily concerned with R-R transfer.) This multi-line cascading prevents adequate control of multi-line or single-line operation; and it needs to be understood if chemical lasers are to be properly stabilized. Accordingly, we feel that this is a good program which should be perpetuated, even though the role that chemical lasers may play in NASA's future is not certain.

Another effort addresses the difficult problem of attaining visible lasing with purely chemical pumping. NASA and DoD have jointly supported Cool's shock tube experiment in which metal compounds are rapidly preheated by a shock, supersonically mixed with an oxidizer and then stimulated by an E-beam which initiates chemical reactions. Cool is optimistic about Scandium Fluoride as a lasant, but thus far no purely chemical pumping in the visible has succeeded.

Cool is also experimenting with dissociation of metal compounds by electric discharges. This work is jointly funded by NASA, ARPA (ONR) and AFOSR. ARPA, in particular, has sponsored many such programs. The results are available in the literature. All DoD efforts thus far have peaked at 15KW power levels because of the difficulty of producing sufficient metal vapor from the electric discharge (We will later discuss NASA-supported work at JPL which relates to this work).

Finally, some excimer research is being supported by NASA also at Cool's lab. Studies of Krypton Fluoride are underway, looking toward the possibility of excitation by high energy photons or nuclear particles. Another idea involves interactions of metastable Argon with OH molecules.

The need for high power visible lasers is so great that any promising approaches must be carefully explored. The High Energy Molecular Laser programs described in the preceeding paragraphs are sufficiently interesting that they should be continued unless proven unfeasible.

C. SUMMARY OF LANGLEY FINDINGS

Our overall impressions of the Langley laser research program are fraught with considerable skepticism. The formal judgements resulting from our evaluations are summarized in Table III-1; but the scoring shown in the table is weighted considerably by our belief in the value of certain concepts (such as Large Volume Preionization and Chemically Pumped Visible Lasers), more than by actual accomplishments to date. We are made cautious by the apparent circumstance that the Langley Laser and Molecular Physics Branch is a seedbed for new ideas, many of which do not germinate. It is apparent that Langley supports both Javan and Cool, but it is not clear how the twenty man years of in-house effort at Langley either contributes to this work or produces independent results at a very significant level. The key to a successful program is to first identify truly useful concepts and then to see them through to reality.

The new focus on photonics programs at Langley should provide an opportunity for greater identity on the part of individual researchers. We have included "New Photonics Efforts" favorably in Table III-1 because of our strong belief in the importance of this program. New methods of power conversion, transmission and storage will have large impacts on NASA's future efforts; and we hope that positive contributions will emerge directly from NASA's research centers. Since similar work is also underway at Ames and Lewis, it is important that good coordination be established and maintained.

Table III-1
EVALUATION OF NASA LANGLEY LASER AND MOLECULAR PHYSICS BRANCH

RESEARCH PROGRAMS / EVALUATION CRITERIA	Application Identification	Uniqueness	Relevance to Other Programs	Physical and Technological Feasibility	Overall Significance for NASA	Overall Mean Value	W. J. Schafer Recommendation
High Pressure CO_2 Tunable Lasers	1	3	1	2	1	1.6	T
Large Volume Preionization	3	3	3	3?	3	3.0	E
Javan's Support	1	2	3	3	2	2.2	S
Atmospheric Transmission Studies	0	1	3	3	1	1.6	T
Chemical Laser Instabilities	2	1	2	2	2	1.8	S
Chemically Pumped Visible Lasers	3	3	3	1?	3	2.6	S
Excimers	3	1	3	2?	3	2.4	S
Cool's Support	2	2	3	3	3	2.6	S
New Photonics Efforts	3	3?	3	2?	3	2.8	E
OVERALL MEAN SCORE	2.0	2.1	2.7	2.4	2.3	2.3	

Significance Levels:
0 = None
1 = Small
2 = Fair
3 = High

Recommendations:
T = Terminate the program
S = Sustain at present level
E = Expand the program

CHAPTER IV. RESEARCH AT LEWIS RESEARCH CENTER

A. INTRODUCTION

The laser research situation at Lewis contrasts sharply with that at both Langley and Ames. The Lewis program conveys the impression of a highly organized and integrated effort having a clear central technological theme and a distinct systems flavor. Lewis is also very hardware oriented. It is very evident that the Lewis staff includes some extremely competent engineers who are capable of delivering excellent results. Regarding the execution of the various programs, we have no criticisms concerning the adroitness with which the work has been accomplished. We can (and do), however, question some of the basic motivations and justifications which underlie the work.

Before beginning the critique, it is relevant to recall that Lewis has been given the role of Lead Center* for all of NASA's lasers systems technology and large adaptive optic systems planning. Direction and coordination of the activities of the other NASA centers remains a prerogative of NASA Headquarters, but Lewis has the responsibility of maintaining and expanding programatic and technical communication with the various NASA centers and with other agencies such as ARPA, AFWL, MICOM, NRL, SAMSO, and ERDA. In addition, Lewis periodically reviews high power laser programs being pursued by major DoD contractors such as UTC (Research Labs and P & W), Avco, TRW, Hughes, Rocketdyne, and Northrop. The central objective

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*There appeared to be some confusion in defining responsibilities that accompany this title, and we feel that Headquarters should clarify this point.

as stated by Lewis is, "To define and evaluate, based upon in-depth research and technology, the potential of high power lasers, and to recommend their appropriate future applications". The program responsibilities include: (1) development of an understanding of the relationship between the capabilities of high-powered lasers and NASA's future needs, (2) identification and evaluation of critical technology areas, and (3) realistic definition of potential applications. Lewis also has stated that it hopes to give unity and direction to the laser effort, while taking a broad and flexible approach. It wishes to avoid duplicating the work of other agencies while pushing laser technology ahead on a broad front.

One function of W. J. Schafer Associates in the present review is to make an appraisal of the success with which Lewis is accomplishing these goals. In the pages which follow, we shall attempt to comment briefly both upon technical aspects of the work and also upon the relevance of the work to the perceived goals.

B. CARBON DIOXIDE CLOSED CYCLE CW LASER PROGRAM

The largest single project in the entire NASA laser research effort is the CO_2 closed cycle CW Electric Discharge aser at Lewis. This laser, which is currently operating at a power level of about 6 KW, has been built entirely* in-house. Its purposes as described by Lewis, are, (1) to evaluate operational characteristics, power scaling, beam quality, and resonator design, (2) to provide a beam for application experiments, (3) to perform feasibility screening of new electrical

* Major components, such as blowers, for example, were purchased.

excitation techniques for CO_2, (4) to study the evolution of contaminants in a completely closed system, and (5) to evaluate scale-up potential and design features of GDL/EDL flight-type closed-cycle CW systems. Particular features of the laser which make it unique are its ability for completely closed-cycle operation and for long-duration sustained CW operation. It also has a very flexible cavity design to permit further resonator research, a near state-of-the-art computer control and data acquisition system, and a limited target-range capability for application experiments.

The heavy commitment to CO_2 technology at Lewis requires us to expand the CO_2 debate still further. In Sections II B1 & 2 and III B1 we have already expressed our strong reservations concerning the ultimate usefulness of CO_2 lasers for NASA applications. In summary, we feel that the intrinsic inefficiencies of CO_2 coupled with the extrinsic inefficiencies resulting from the need to build large optical systems to accomodate the longer wavelengths combine to make CO_2 unacceptable as a candidate for any serious application considerations. The additional problem of high atmospheric absorption (Figures II-1 & 2) further curtails our interest in CO_2 for any ground-to-space uses, particularly when we consider the restricted range of accessible zenith angles. CO always outperforms CO_2 from a good location--i.e., a site having a small amount of total precipitable water vapor. H_2O is the only significant absorber of CO, while CO_2 resonance absorption extends to very high altitudes.

The foregoing views were expressed by W. J. Schafer Associates at the NASA Program Review in Cleveland, Ohio on April 21, 1976. Lewis has rebutted with the following statement:

> "It is apparent that NASA high-power laser missions such as propulsion or power transmission require long-duration continuous-wave operation - which implies a closed-loop system. The decision to work with the CO_2 laser system is based upon several factors. Most important is the fact that today, tomorrow, and in the immediate future, CO_2 is the only high-power laser for extended continuous-wave operation. This decision is reinforced by the recently initiated AFWL Short Range Applied Technology (SRAT) Program. CO_2 was chosen as the Lewis high power laser because it was less complex than a CO system, presented an opportunity for technology advancement, and offered less of a technology risk than any other existing system. The hands-on experience from this device could be achieved in a shorter period of time and will be helpful in future decisions regarding closed-cycle systems. The facility was designed as a closed high-vacuum system so that the effects of contamination and laser chemistry could be investigated in a system much cleaner than what had been previously available. These results should provide guidance for future CO_2 systems".*

At first glance, there are some very reasonable points in favor of this position, particularly with regard to the value of "hands-on" experience. At the risk of seeming obdurate, however, we feel that several debatable issues must be broached.

First, let us examine the frequently repeated key premise that NASA's missions will require closed-loop, CW operation. The closed-loop idea is suspect, for example, because it appears quite possible to leave the laser on the ground as would be required

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*Private communication from Heads of Laser Engineering and Laser Technology at Lewis.

by surface-to-surface propulsion any way. For space application, the beam can be bounced off cooperative relay mirrors in orbit. The prime justification for this concept is provided by the expectation that it will be relatively easy to develop large-scale adaptive optics and, thus, to build very large projection apertures both on the ground and in space within the not-too-distant future. This probably will involve a time frame much shorter than would be required to develop high powered lasers for space deployment. It is clearly possible that, given the successful development of adaptive optics, single-and-multiple bounce beam transmission can be utilized to deliver a nearly diffraction-limited beam to any point in cislunar space. Thus, it should be apparent that necessity for closed-loop operation of the laser is no longer certain. In fact, some concept for air-breathing lasers now being investigated might lead to very substantial open-cycle fuel economies. Since a major NASA applications area is ground-to-space propulsion, anyway, it is not at all clear why the inefficiencies associated with closed-cycle operation should be regarded as a necessity.

As to the stated necessity for "CW" operation, it is, of course, possible that NASA's applications might require <u>long duration</u> power input. But this does not necessarily equate to continuous wave operation. In fact, there are possible advantages to be gained by operating continuously in a multi-pulse mode. These advantages embrace various aspects of laser physics ranging all the way from increased availability of lasing transitions, through superior atmospheric propagation, to improved energy coupling at the receiving end. The tradeoffs between CW and pulsed operation are the subject of much detailed scrutiny by DoD, and the decision as to which is best for a given application is by no means trivial or obvious.

Even if CW operation is essential for NASA's applications, we still disagree with the Lewis statement that "today, tomorrow, and in the immediate future CO_2 is the only high-power laser for extended continuous wave operation". Flowing gas CO lasers are already operating at Northrop and AVCO in a quasi-CW mode at extremely high power levels. Sustained operation is to be demonstrated in the near term.

Continuing with our comments on the Lewis statement, we strongly disagree with the contention that the decision to resort to CO_2 as a near term NASA expedient is reinforced by AFWL's use of CO_2 for the Short Range Applied Technology (SRAT) Program. SRAT is, above all else, *short range*, both in terms of the distance that the beam must propagate through the atmosphere and the need for a flyable "all-up" system in the near future. (The availability of a lightweight large power source such as an MHD generator, for example, could conceivably lead to a flyable CO system in the same time frame!) These problems have absolutely no connection with NASA's, since the technology requirements are completely different from start to finish.

Next, we question the justifications of the program based upon quick opportunities for "technology advancement" and "minimum technology risk". This is a very poor argument for undertaking and sustaining a major research effort. Technology advancement is meaningless if the advance is directed into an area which has no ultimate usefulness. Indeed, this might be a "low-risk" approach; but, as in the case of most investments, it is likely to produce small returns. First, one should figure out what one is trying to do and then ask what is the best way to do it when all factors are taken into account.--NASA is familiar with a classic example of these two modes of approach: In the late 1950's the U.S. made the daring decision to develop and exploit liquid hydrogen as a fuel for rockets. The Soviets took the more conservative "low-risk" option of simply scaling up LOX-Kerosene technology. Our approach allowed us to achieve our goal of manned landing on on the moon within a decade, while theirs provided a short term, limited payload, benefit.

For all of the foregoing reasons, we feel that W. J. Schafer Associates can best serve NASA's mandate for us to express our most frank opinions by adhering to our position that CO_2 technology is inferior technology in the context of NASA's goals for high energy lasers. We are well aware of the amount of excellent work that has gone into the development of the Lewis CO_2 facility; and we have a strong conviction that this background will prove to be most valuable in terms of the experience accrued by the personnel involved. We definitely do not believe, however, that the future will be best served by an extended developmental effort based upon this machine. Rather, we believe that it should be modestly sustained as an experimental facility devoted primarily to applications testing. Such experiments might include materials processing, laser energy conversion, and, most particularly, laser propulsion. Theoretical studies of laser propulsion may have progressed to the point where a demonstration of laser-induced thrust generation is desirable. It may also be worthwhile to investigate the possibilities for using the facility as a testbed for other lasing species, hopefully at shorter wavelengths. Projects should not be pushed into the experimental phase, however, until the need for an experiment is well established. Otherwise, the all-too-frequent phenomenon will occur wherein much money and the talents of many people are invested in laboratory exercises that can only be classed as gadgeteering. --Over and over again it is necessary to ask oneself the question, "Why am I *really* doing this thing"? Insufficient reverence for this query leads to the launching of Crusades to find the <u>wrong</u> Holy Grail!

C. TRANSMISSION AND PROPAGATION STUDIES

As at the other NASA centers, a number of smaller research projects are supported by Lewis, both as in-house programs and as contracted outside efforts. Table IV-1 shows all of the

TABLE IV-1

LEWIS RESEARCH CENTER CONTRACTED LASER-RELATED STUDIES

Lewis Program	Contract	Contractor	Total NASA Funding
High Power Laser Facility Support	IR Interferometer for Plasma Diagnostics	Westinghouse Research Corporation	$ 93.4K
Laser Transmission Technology	Standardization of Radiation Measurements (Joint study with NBS, NASA & DoD--total program $350K)	National Bureau of Standards	35K
	Conceptual Design and Analysis of Large, Space Based Laser Transmitters	Itek Corporation	66K
	Analysis and Design of an Adaptive Phased Array	Rockwell International	200K
	Analysis and Design of Phase-Locked Lasers	(Funds committed; contractor not specified)	--
	Feasibility Determination of a Thermoelectronic Laser Energy Converter (TELEC) System	Rasor Associates	35K
Excimers & Visible Lasers	Physical Processes of Excimers	Univ. of Illinois	45K
	Electrically Pumped Excimer	Hughes Research Lab.	200K
Laser Propulsion	Laser Absorption in Flowing Gas Devices (Experimental Study)	Avco Corporation	161K
	Absorption Mechanisms Analysis (Analytical Study)	Physical Sciences Inc.	36K
	Laser-Heated Rocket Absorption Study (Analytical Study)	Physical Sciences Inc.	75K
	Laser-Heated Rocket Thruster (Analysis, Design, and Fabrication)	Rocketdyne, Inc.	98K

contracted work. In this Section the *Transmission and Propagation* studies will be briefly reviewed.

1. <u>Establishment of High Power Laser Measurement Standards</u>

This work is being performed at the National Bureau of Standards in a collaboration among NASA, DoD, and NBS. NASA is contributing only a small fraction of the total funding ($35 K of $350K). This very important work must be done; and it is proper for NASA to support it and to be identified with it. The level of funding seems reasonable and consistant with the level of NASA's need for calibration standards at the present time.

2. <u>Conceptual Designs of a Large Space Aperture</u>

The initiation of this program to investigate the feasibility of large projection apertures (up to 30 meters in diameter) in space was a master stroke on the part of Lewis planners. They correctly identified a key element in all space laser concepts that has been insufficiently studied by interested parties at both DoD and NASA. The study, carried out by Itek Corporation, establishes the credibility of large, diffraction-limited mirrors deployed in orbit. Of three concepts studied, a segmented mirror partially assembled in space was found to be the most practical. Active (adaptive) mirror surface control will be required; and the study concluded that actuator, sensor and control logic requirements can be met without new technology development. We feel that this last point deserves more detailed scrutiny, because it is likely that advanced control methods can reduce the weight and complexity of the structure, facilitating easier errectability. Full adaptive control would also make possible a fully cooperative relay mirror for the concept discussed in Section B. There is also a need for more knowledge of relevant materials properties.

This very important program deserves one or more follow-on studies. In addition to the recommendations contained in the Itek final report, we believe that a largely neglected need resides in the area of scaling relationships. Math models need to be developed for several aperture diameters so that parameters of interest such as weight, natural frequency and optimum element size can be scaled for various applications. (Itek's report gives very little attention to weight and how it scales.)

Itek's study barely touched upon alternate optical designs. These should be investigated in greater depth, and a comparison of candidate configurations should be made on the basis of sensitivity and fabricability, including alignment and test. Other configuration tradeoffs should be examined also to compare weight, natural frequency, ease of deployment, sensitivity to environment, and most certainly cost, at least on a relative basis.

Here is a clear opportunity for NASA to establish pre-eminance in a field of enormous significance, not only for space laser applications but also for astronomy and surveillance.

This Lewis study has already had considerable impact at both ARPA and SAMSO, and its implications are becomming widely recognized in other DoD agencies.

3. Analysis and Design of a Ground Based High Power Laser Adaptive Phased Array

The very important concept of optimizing propagation of a laser beam through the atmosphere to space by the use of adaptive optics was not primordially originated at Lewis Research Center; but, as in the case of the Large Space Aperture, Lewis was the first laboratory to "take the bull by the horns" and actually undertake a feasibility study. (In fact, there has been so much preliminary work done on related problems by DoD and its contractors that we incorrectly stated at the April Program Review that we felt that Lewis was largely duplicating other DoD efforts. Our views were distorted by our perception of DoD's level of interest in this idea*. To our knowledge, the Lewis study is the first formal unclassified study that addresses the problem of detailed analysis and conceptual design of a system for beaming power from the Earth's surface to space.)

In section IVB we have already stated the strong arguments in favor of transmitting a beam from earth to a cooperative space mirror, and we have indicated our belief that this idea may be of high value to NASA. Lewis' study of the phased-array transmitter, of course, relates closely to this concept. The whole problem of how to transfer large amounts of power from the ground to space is centrally important for many applications of great interest

*Both the ARPA Space Object Identification (SOI) Program and Laser Technology Identification (LTI) study have dealt with this class of problem in some detail.

to both NASA and DoD. We feel that NASA needs to establish a close working relationship with ARPA to avoid duplications of effort and to move the concept forward to the test phase in the most expeditious possible way. In this context, we think that NASA needs to look a bit more closely at its own intended applications so that a clear area of interest can be defined. This surely seems to be another activity where NASA can very properly assume a leading role in a high-payoff scenario which can capture the imagination of the public.-- WJSA has found in an unpublished study that large economics might accrue from laser propulsion when very large amounts of material need to be raised from the Earth to high orbit, as would be the case for the construction of the "L-5" space station. The L-5 concept has captured a surprising amount of attention and acceptance from a segment of the general public. NASA should be able to take advantage of this interest.--As a program addressing applications such as *Propulsion*, this work should have much more emphasis and other areas (e.g. the Closed Cycle CO_2 Laser) much less.

4. Analysis and Design of Phase-Locked Lasers

The idea of phase-locking numerous lasers to achieve higher intensity on target while relaxing the demands upon individual lasers (and associated optics) is a very good one. Here is another concept which has been accepted rather passively and intuitively by the DoD community, while Lewis proceeded to do something about it. The possibilities for in-house phase-stability experiments at Lewis plus the areas of concentration considered in the Rockwell study contract should lead to some new insights.

Again, we caution that this sort of project should be tied to an application at the earliest possible moment to avoid straying into unproductive areas. Because of the limited funds available, Lewis should concentrate on a small, <u>unique</u> program. (There is a tendancy within NASA to invent $500K schemes for $50K budget blocks!) Nevertheless, the concept is very important and should be furthered.

5. <u>Feasibility Study of TELEC System</u>

The Thermoelectric Laser Energy Converter (TELEC) is one of the devices initially studied under an Ames contract (see Tables II-1 & 2). A follow-on contract to Rasor Associates has been initiated by Lewis Research Center to (1) perform parametric analysis of megawatt level TELEC devices, (2) perform conceptual design evaluations for 1 to 10 megawatt TELEC systems, and (3) determine feasibility of a 10 KW TELEC for actual testing with the Lewis CO_2 CW laser. If the study indicates feasibility, a preliminary design of a test cell will be undertaken.

The TELEC concept is quite interesting because it has already been tested at 35% conversion efficiency with RF heating, and it has a potential conversion efficiency as high as 50% with laser stimulation. It can be used not only for NASA applications but also for such needful considerations as toppingcycle utilization of waste heat from steam power plants. Experimental tests will also be useful to establish scaling laws for high power TELEC cells.

The TELEC study is definitely a proper province for Lewis research. It is nicely suited for a test on the CW CO_2 laser facility. We think, however, that the test scenario should be kept simple. An unofficial Lewis proposal to test the TELEC in a more complicated scenario using adaptive optics to correct for disturbances along an atmospheric path smacks of unnecessary gadgeteering. The important thing for the present is to prove the power conversion concept. Adaptive optics beam correction techniques are being extensively investigated by DoD.

6. Beam Shaping for Maximum Power

Even at major laboratories it is sometimes necessary to perform exercises which have educational value. We assume that this classical review of one of the more important aspects of optical engineering will be useful for Lewis decision makers.

D. ADVANCED LASER CONCEPTS

Here we lump together a few "add-ons" to the High Energy CO_2 CW Laser Program plus other work at Lewis pertaining to advanced laser concepts.

1. Screening of New Electrical Excitation Methods for CO_2

Being tied to the CO_2 concept, this project has little value if our assessment of CO_2 is correct. If we are wrong, it may be a worthy experiment. As long as the CO_2 facility is continued, perhaps it makes sense to try to improve it on a limited basis.

If the applications evaluation aspect of the CO_2 project is emphasized, it may be desirable to increase the power of the device. Thus, limited experimentation with multiple-pass beams and E-Beam excitation might be justified.

2. Evaluation of Scale-Up of the CO_2 Closed Cycle System to a Flight System

The work which has been done already seems to provide ample examples of the futility of pursuing this course. The low efficiency of CO_2 lasers graphically manifests itself when one begins to consider putting such systems in orbit! The weight in orbit for lasers with interesting power levels ($\sim$10MW) becomes tremendous ($>10^6$ lbs), even ignoring the weight of the power source and heat dissipation system.

3. Gas Contamination Study

This study is unique in spite of its intimate connection with the closed cycle CO_2 technology. As long as the facility is operating, it makes sense to obtain this data. It may be relevant to other types of electric discharge lasers.

4. Excimers and Visible Lasers

As we have indicated in previous Chapters, we generally approve of research that may move the state of the art of laser physics ahead. Even though many different laboratories are working on new short wavelength laser concepts, it is clear that many different lines of investigation must be explored. The Lewis programs are focussed upon the basic physics of CW excimers and upon large-volume ionization concepts. Both of these are important. We have already remarked in our commentary on the work at Langley Research Center that we regard the volume ionization research as being of highest importance. The CW excimer work is unusual, because most laboratories are pursuing pulsed methods. Both projects should be sustained or expanded.

5. Exploratory Technology Applications Studies

The four principal conceptual areas upon which Lewis is concentrating are, (1) power transmission and conversion, (2) laser propulsion, (3) laser-materials interactions, and (4) photochemical reactions. Efforts are underway to define design and scaling parameters for promising application systems. Table IV-2 shows a breakdown of near and far term intentions of the program.

Basically we find nothing wrong with the intended program. We think, however, that it should be greatly accelerated, because the applications will certainly drive the technological needs. Since ARPA is spending a lot of money in similar areas, there is a strong need to develop cooperation and coordination.

TABLE IV-2

LEWIS EXPLORATORY TECHNOLOGY APPLICATIONS STUDIES*

Concept	Near Term Programs	Long Range Targets
Propulsion	(1) Energy Absorption/Scalability Experiments (2) Thrust Experiments (3) Thruster Scaling to 1 MW (Design, Fabrication & Testing) (4) Extended Analytical Model	(1) By FY79: Establish concepts for efficient transfer of laser energy to a working gas (2) By FY80: Assess Laser Propulsion System components concepts (3) By FY80: Determine the economic and mission effectiveness of laser propulsion
Materials Processing	(1) Investigate use of high powered IR holography for precision laser machining (2) Explore uses of photon-induced catalysis (3) Initiate additional materials processing studies	(1) By FY80: Identify and evaluate non-propulsive applications for high power laser systems
Photochemistry	(1) Extend studies of photocatalysis	
Power Conversion	(1) Systems Studies (2) Laboratory Experiments	

*From data supplied by Lewis Research Center at NASA Program Review, April 1976

E. SUMMARY OF LEWIS FINDINGS

Table IV-3 shows the synoptic breakdown of our evaluations for Lewis Research Center. It can be seen that we have expressed high opinions of nearly all programs except those related to the CO_2 effort. In spite of the low scores in the CO_2 area, the overall results of the evaluation are quite favorable. Lewis has some very good people, and, on the whole, an excellent research program.

Table IV-3
EVALUATION OF NASA LEWIS HIGH POWER LASER PROGRAM

	RESEARCH PROGRAMS / EVALUATION CRITERIA	Application Identification	Uniqueness	Relevance to Other Programs	Physical and Technological Feasibility	Overall Significance for NASA	Overall Mean Value	W. J. Schafer Recommendations
CO_2 CLOSED CYCLE CW PROGRAM	Evaluate Basic Characteristics of Operating Laser	1*	2	1*	3	1*	1.6	S
	Screen New Electrical Excitation Methods	1*	2	2	3	2	2.0	S
	Evaluate Scale-Up to Flight System	1*	3	1*	1*	0*	1.2	T*
	Gas Contamination Study	3	3	2	3	2	2.6	S
TRANSMISSION & PROPAGATION STUDIES	Establish High Power Laser Measurement Standards (NBS)	3	3	3	3	2	2.8	S
	Conceptual Designs of Large Space Aperture (Itek)	3	3	3	3	3	3.0	E
	Analysis & Design of Adaptive Phased Array (Rockwell)	2	2	3	3	3	2.6	S
	Analysis & Design of Phased-Locked Lasers	2	2	3	3	3	2.6	S
	Feasibility Study of TELEC System	3	2	3	3	3	2.8	S
	Beam Shaping for Maximum Power	3	0	3	3	3	2.4	T
	Excimers and Visible Lasers	3	2	3	3?	3	2.8	E
	Exploratory Technology Applications studies	3	3	3	3?	3	3.0	E
	OVERALL MEAN SCORE	2.5	2.1	2.5	2.8	2.3	2.5	

Significance Levels:
0 = None
1 = Small
2 = Fair
3 = High

Recommendations:
T = Terminate the program
S = Sustain at present level
E = Expand the program

*Because it is tied to CO_2 concept

CHAPTER V. LASER RESEARCH AT JET PROPULSION LABORATORY

A. DESCRIPTION OF PROGRAM

A small but important fraction of NASA's laser research program resides at Jet Propulsion Laboratory. The work centers entirely upon the problem of how best to develop high power lasers for short wavelengths. Almost all of the work at JPL is concerned with metal halide lasers. The copper chloride trimer, $Cu_3C\ell_3$, has emerged as a very promising candidate because it is capable of both high pulse rate and high average power. JPL is taking a global view of other halide options, however. Sophisticated chemical kinetics computer codes have been developed, and screening of many other metal halides is in progress.

The lasant in metal halide lasers is simply atomic metal vapor. Copper is a good atom because it appears to have a minimum of parasitic loss mechanisms, and hence highest energy output and efficiency. (It is an interesting fact that the very first laser ever attempted utilized copper vapor as the lasant. Unfortunately, the experiment was unsuccessful). The big halide breakthrough has been the reduction of the lasant operating temperature from $2000^{\circ}K$ for pure copper to $400^{\circ}K$ for copper chloride. An initial electric pulse dissociates the $Cu_3C\ell_3$ molecule, yielding the metal vapor. Then, a second pulse is applied to establish a population inversion in the dissociated metal vapor. This double pulse technique was pioneered by JPL, and it constitutes important progress. Subsequent pulses continue to produce lasing, provided that the pulse spacing and lasant temperature are maintained. The laser output is quite sensitive to both of the latter parameters.

The work described above has been jointly funded by the Navy (with ARPA money) and Los Alamos Scientific Laboratory (with ERDA money) in addition to NASA. JPL does not distinguish technically between the efforts for these three agencies at this point, because the program still can only be categorized as basic research. It is clear, however, that the ultimate applications requirements for the three agencies will differ considerably. The Navy is particularly interested in underwater communications and imaging (using the 5106 Å green radiation of copper), and LASL wishes to exploit the possibilities of this laser for isotope separation. These applications will require high peak intensities, but not necessarily high average power, whereas NASA's applications, as presently understood, will definitely require high average power. The work presently in progress will lead to a copper chloride laser having an average power of ∿ 100W at a pulse rate of 10^4 pulses/second. Each pulse has a width of ∿ 30nS.

JPL is eager to move ahead into the multi-kilowatt regime. A powerful closed-cycle copper chloride device would probably recondense the $Cu_3C\ell_3$ and run the liquid through a radiator for cooling. A subsonic flowing-lasant system would be fairly simple to devise, but the more complex supersonic system would permit faster pulse rates (∿ 10^5 pulses/second) and, hence, higher powers. JPL feels that it could move forward rapidly into this sort of program if funds became available. This would, of course, make metal halides the state-of-the-art for visible lasers, far surpassing the present state of excimer and heterodyne laser development.

The only other area of laser related activity at JPL which is receiving NASA support is a joint program with LASL to study nuclear pumped lasers. The investigation is concentrating on lasing He_3 at 6400 Å by exposure to a flux of 10^{18} neutrons/cm^2/second from the Godiva pulsed reactor. This is a very crude experiment compared with the conceptual nuclear system, which would make use of the tremendous power density inside a gas-core reactor.* Nevertheless, the possible payoffs from successful development of a means to generate laser emission directly from that environment are tantalizing to contemplate. Present EDLs have mass-flow efficiencies of $\sim$ 10 KJ/lb., and chemical lasers are operating in the range from 50 to 100 KJ/lb. The nuclear laser might have an efficiency measured in Megajoules per pound rather than Kilojoules.

B. COMMENTARY

1. Metal Halide Lasers

The technical aspects of the metal halide laser are difficult to discuss beyond the publications and reports of the JPL research group. Indeed, they have led the field since the inception of this idea, and we believe that their assessments are accurate. The kinetics theory of this type of laser is extremely

* We understand that recently both the Sandia Labs and Langley Research Center have also conducted experiments on nuclear pumped lasers.

difficult and not completely understood. At present the experimental data is more advanced than theory. Nevertheless, several things are apparent, particularly with regard to efficiency and scalability.

The efficiency of the present devices stands at about one percent, making them comparable with many CO_2 lasers in this respect. But, as we have previously mentioned, the overall efficiency of a system built around a short wavelength laser will be much greater than that of a long wavelength laser because of the much smaller optics required to deliver a specified irradience to the target. Furthermore, the ultimate efficiency of the metal halide devices is expected to reach at least 3% and perhaps as much as 10%.

Scalability seems insured, although detailed scaling laws are not yet in hand. There is a complex interaction between lasing cross section and pulse delay time, and the optimum temperature of the lasant is quite critical: With all other parameters held constant, the optimum delay time decreases as the discharge tube diameter decreases, indicating that diffusion as well as electronic deexcitation is acting to deplete the lower levels between current pulses. This relationship is, of course, different in flowing gas devices.

With all of the uncertainties, it is still clear that the promise of metal halide lasers is at least as favorable as that of excimers, and the time of availability may be much nearer for metal halides. It is also clear that it will be very difficult to make direct photolysis work for chemical lasers at visible wavelengths*. Hence, the outlook for metal halides cannot be understated.

In previous chapters, we have reiterated many times our belief that successful development of short wavelength high power lasers is absolutely essential if large-scale exploitation of lasers for space applications is to be realized. The JPL metal halide program appears to be one of the brightest hopes for near-term realization of this necessity and, hence, should be very actively supported. The funding level for this program has actually remained constant for several years in spite of steady technical progress. Inflation is actually reducing the level of support. Moreover, since the same research is being supported under two different NASA RTOPs, further loss is occurring because of administrative necessities such as duplicate report writing. --It is a bit paradoxical that NASA wants to support basic research and futuristic applications concepts but puts much of its money into hardware (e.g. the Lewis CO_2 laser) and peripheral efforts which do not focus on specific applications concepts. The JPL high power metal halide program provides a perfect example of a project which has all of the credentials that NASA should desire and, yet, is not being pushed ahead with adequate enthusiasm. It would be equally valuable to other agencies, and NASA would be preeminent in the field.

*There is some hope that hybrid systems may be developed which combine chemical photolysis with other energy inputs such as solar or electrical energy.

2. Nuclear Lasers

The modest experimentation that has been done with nuclear particles as a pumping source for lasers can scarcely be regarded as definitive. It seems to us that experimentation is almost premature in this field. Detailed theory and computer modelling seem more in order. The JPL laser team has nuclear physicists and theoreticians who are capable of launching the needed studies. We feel that an effort should be sustained in this field, but we do not hold much hope for usable systems within the foreseeable future. As long as a viable possibility remains that efficient nuclear lasers can be built, however, the research must continue. There is sufficient justification for NASA applications that it seems logical for the funding to continue.

3. Future Prospects

JPL has a fine team of physicists and engineers who are a real asset for NASA. At present, however, they are very manpower limited and cannot realize their full potential. They desire to expand beyond present programs, with particular interest in hybrid laser systems which might combine chemical energy with electric, solar, or nuclear inputs. We strongly endorse the capabilities of this team, and hope that NASA will consider augmenting it both with funds and manpower, perhaps by transferring focus from another center. Our evaluation summary is appended in Table V-1.

Table V-1

EVALUATION OF NASA JET PROPULSION LAB LASER RESEARCH

RESEARCH PROGRAMS / EVALUATION CRITERIA	Application Identification	Uniqueness	Relevance to Other Programs	Physical and Technological Feasibility	Overall Significance for NASA	Overall Mean Value	W. J. Schafer Recommendation
Current Copper Chloride Laser Research	3	3	3	3	3	3.0	E
Metal Halide Screening	3	3	3	2	3	2.8	E
Design of High Power Super Sonic Closed-Cycle Copper Chloride Laser	3	3	3	3	3	3.0	E
Develop Master Oscillators for HP Cu_3Cl_3 Lasers	3	3	3	3	3	3.0	E
Nuclear Laser Technical Support	3	3	3	1?	3	2.6	S
Nuclear Laser Experimental Support	3	3	3	1?	3	2.6	S
Proposed Hybrid Laser Research	3	3	3	2?	3	2.8	E
OVERALL MEAN SCORE	3.0	3.0	3.0	2.1	3.0	2.8	

Significance Levels:
0 = None
1 = Small
2 = Fair
3 = High

Recommendations:
T = Terminate the program
S = Sustain at present level
E = Expand the program

CHAPTER VI. RELATIONSHIP OF NASA TO THE WORK OF OTHER AGENCIES

A. INTRODUCTION

In this chapter we shall briefly outline high power laser technology areas which are of mutual interest to both NASA and other agencies. The underlying motive is to encourage mutually beneficial cooperation and problem identification. The surest way to accomplish this is to identify very carefully those things which are unique. Consequently, we shall omit from the following discussion the several cases already explained wherein there is duplication of effort; and we shall concentrate instead upon the unique topics which seem ripe for collaborative exploitation.

Uniqueness can characterize either complete research efforts, pieces of research efforts, or entire applications. For example, the JPL metal halide program is unique physical research in the short wavelength laser field; the Lewis Large Space Aperture study is a unique part of the technological research leading to the exploitation of lasers in space; and the building of an L-5 space colony is a unique NASA concept which may require laser propulsion from the Earth's surface in order to be feasible.

B. CRUCIAL TECHNOLOGIES

The following list of technology areas is offered to illustrate some topics of outstanding mutual importance for many purposes, providing ample opportunities for unique contributions and cooperative efforts relating to on-going projects.

(1) Large Optics: Beyond the pioneering study of large erectable optics inspired by Lewis Research Center, there is a vast amount of work to be done. In Chapter IV C.2 we have already outlined numerous follow-on study topics derived from the initial work. In the broader view, the work will become much more explicit when it is tied to specific applications, leading to large individual programs.

Soon, systems studies should be undertaken to identify the specific applications and to separate the common requirements from unique requirements. Such systems studies could very properly be undertaken as joint efforts by NASA, ARPA, and the Air Force, for example. This could lead directly to cooperation rather than competition in the future.

(2) Adaptive Optics: It is perfectly evident that adaptive optics will play a crucial role in the realization of Large Optical Systems. It is, nevertheless, entirely proper to regard Adaptive Optics *per se* as a separate technology area. The number of optical elements, size and weight of the elements, amplitude and frequency response of the adaptation, control method, software requirements, and a host of other problem areas will be peculiar to each application. Requirements will be enormously different for adaptive systems projecting from the ground to space, from space to space, and from space to the ground. A whole new technology and many new industrial opportunities will grow out of these needs.--One of the key areas which needs attention is how to extend adaptive control to thousands of surface elements without monstrous computing systems. Simplicity is crucial.

(3) Short Wavelength Lasers: The advantages to be realized from short wavelength lasers operating at high power levels are so tremendous that this quest must be regarded as one of the most exciting in the history of technology. Many promising avenues exist, but it is quite possible that the best ones have not yet been found. Any unique idea which survives initial peer review and careful theoretical scrutiny is worth pursuing experimentally.

(4) Very High Power Lasers: The conceptual gap between technological monstrosities which *might* produce large amounts of coherent radiation and truly efficient devices which can do useful and justifyable work is enormous. Recognition of this fact is very important; but it is not apparent that many laser technologists are aware of it. Scaling laws, estimated total system weights and volumes, and estimated total system efficiencies must be regarded as indispensible fructifying principles which govern the evolution of lasers from research projects to application components. Seeking this knowledge should be a major occupation of high power laser enthusiasts.

(5) Kinetics: The difficult physics and chemistry of the lasing process can be understood in depth only by bringing together detailed knowledge from numerous disciplines. Thermodynamics, hydrodynamics, MHD, spectroscopy, statistical mechanics, electrodynamics, and physical chemistry are a few of the areas of formal knowledge which must be brought to bear. Indeed, the history of high power laser progress has been built around synergistic extrapolations from research in these areas. (We have already mentioned that the first gasdynamic laser concept grew from

reentry physics research). It is likely that the future of lasers will be more influenced by new basic research than by any existing hardware program.

(6) Volume Preionization: As we have discussed at length in other Chapters, the efficient and uniform excitation of large volumes of gas is an important necessity for the development of efficient high power lasers. This requirement applies for many different kinds of lasers. The "seed gas" concept pioneered at Langley Research Center could have wide applicability if proven feasible because it could eliminate the need for electric discharges and other "excess baggage". Such peripheral concepts for improving the overall efficiency of laser systems must be identified and pursued vigorously.

(7) Fine Pointing Accuracy: Clearly, it will be impossible to beam power to any remote location, either for propulsion or for electric power transfer, unless the beam can be steered with microscopic precision. Pointing accuracies of 0.01 to 0.001 microradians are needed, and they will not be easy to achieve. Ultimately, the fine pointing problem will probably turn out to be a subset of the adaptive optics technology. Any simplifying methods, such as cooperative pointing and tracking based on return signals from the target, etc., will be worth investigating.

(8) Laser Induced Thrust Generation: The application area of *Laser Propulsion* covers a multitude of ideas, ranging from air breathing high-altitude Remotely Piloted Vehicles (RPVs) to interstellar probes. First one must decide what one wishes to propel, and whence. Then one must investigate the propulsion

options in great detail. Preliminary studies indicate that laser propulsion will be a tremendously important asset, particularly for lofting very large amounts of material into space (e.g. for constructing L-5), but much remains to be proven. The very first thing that might be done is to demonstrate conclusively that a gas can be heated to a high temperature in a thrust chamber by a laser.

(9) Materials Interaction and Processing: Laser welding and machining are in their infancy. They may remake entire industries. The preliminary studies of holographic machining at Lewis are an important first step. Innumerable ideas and applications remain to be explored.

(10) Photochemistry: Photochemistry may hold the key to the manufacture of incredible new materials. It may also make presently inefficient processes efficient. An efficient means for separating water into hydrogen and oxygen, for instance using solar energy augmented by laser radiation, would be a gift of inestimable value to the world. It would provide a universally available and storable pollution-free fuel.--The use of solar energy to directly energize lasers is also of premier importance. Solar energy may be an ideal source for large volume space laser excitation.

(11) Environmental Monitoring: It is not entirely clear whether strictly passive spectroscopy can provide sufficient information for remote environmental monitoring. (Astronomy has gone far in understanding stellar and planetary atmospheres by passive

observations only). Laser methods may be of considerable value but this needs to be proven.

(12) Energy Conversion: This category is sometimes a subset of other categories such as *Photochemistry* and *Thrust Generation*. It should also exist as a separately identified crucial technology area relating to the generation and storage of electric power induced by laser beams. Any application that has need for electricity in a remote location might ultimately benefit from this technology.

C. NEW CONCEPTS WHICH RELATE TO NASA'S INTERESTS

There are important study areas now being funded by Dod which may bear heavily upon NASA's future plans: We call attention to them here without elaborate commentary; but we urge NASA to search for common grounds and to monitor the progress of these programs.

The Air Force is moving toward the design phase for an actual demonstration of a high power laser in space. Currently, the Space Laser Experimental Definition (SLED) study is being funded by ARPA through SAMSO under two $550,000 contracts. Both contracts address the same task, basically to identify key problem areas and to define the work blocks. They seek to address all component requirements unique to space applications and to note common requirements. Lockheed and Rockwell International have the present contracts.

The subject of large erectable space apertures has been receiving SAMSO attention. Recently three contracts were awarded to Lockheed, Hughes and Rockwell to study methodology and provide design information.

Both Avco and Physical Sciences, Inc. have contracts to study laser propulsion for ARPA.

CHAPTER VII. SUMMARY OBSERVATIONS AND CONCLUSIONS

In the previous chapters we have reviewed all present NASA research activities pertaining to high power lasers and related fields. Measures of importance called *Evaluation Criteria* were formulated to enable a grading of the programs at each NASA Research Center according to potential usefulness to NASA. We can now coalesce these evaluations, which are summarized in the tables at the end of each chapter describing the work at each laboratory, into a ranking showing our estimates of the approximate value of all of the programs. The results are given in Tables VII - 1, 2, & 3. Where appropriate, the tables also list for each program a *Related Crucial Technology* from the list of twelve technologies discussed in Chapter VI.

In comparing tables VII-1, and VII-2, it is interesting to note that the first table recommending <u>expanded</u> programs, tends toward small but exciting and *avant garde* basic research work. Table VII-2, which recommends programs to be sustained at the present level of effort, tends toward more mundane, engineering oriented projects. These observations probably reflect the high value of new ideas in a rapidly evolving field of science. Moreover, they may point to the difficulty of mounting really worthwhile engineering efforts on a very limited budget.

It should be noted at this point that we have tried to avoid, where possible, passing judgement upon the <u>performance</u> of the research groups at the four laboratories. We have tried to concentrate, instead, upon the <u>usefulness</u> of ideas for NASA's long

Table VII-1

ACTIVITIES OF HIGH USEFULNESS TO NASA

--EXPANDED STUDY RECOMMENDED

Laboratory	Program	Value	Related Crucial Technology
Ames	• Laser Energy Conversion	3.0	Energy Conversion
	• Photochemistry	2.8	Photochemistry
	• Harmonic Up-Conversion	2.8	Short Wavelength Lasers
	• Tunable Laser Lab	3.0	Kinetics
Langley	• Large Volume Preionization	3.0	Very High Power Lasers
	• New Photonics Efforts	3.0	Photochemistry Energy Conversion
Lewis	• Large Space Aperture	3.0	Large Optics
	• Excimers and Visible Lasers	2.8	Short Wavelength Lasers
	• Exploratory Technology Applications Studies	3.0	Thrust Generation Materials Processing Photochemistry Energy Conversion
JPL	• Current Copper Chloride Laser Experiments	3.0	Short Wavelength Lasers
	• Metal Halide Screening	2.8	Short Wavelength Lasers
	• Design of High Power Super Sonic Closed-Cycle Copper Chloride Laser	3.0	Short Wavelength Lasers Very High Power Lasers
	• Develop MOPA $Cu_3C\ell_3$ Lasers	3.0	Short Wavelength Lasers Very High Power Lasers
	• Proposed Hybrid Laser Research	2.8	Short Wavelength Lasers Very High Power Lasers

Table VII-2

ACTIVITIES OF MODERATE USEFULNESS TO NASA

--SUSTAINED STUDY RECOMMENDED

aboratory	Program	Value
Ames	• COEDS Laser	2.8
	• Theory of Vibrational Energy Transfer	3.0
	• Quantum Electronics Theoretical Support	3.0
Langley	• Javan's Support	2.2
	• Chemical Laser Instabilities	1.8
	• Chemically Pumped Visible Lasers	2.6
	• Excimers	2.4
	• Cool's Support	2.6
Lewis	• CO_2 Closed-Cycle CW Laser--Basic Evaluation	1.6
	• Screen New Excitation Methods for CO_2 Closed-Cycle Laser	2.0
	• Gas Contamination Study	2.6
	• Establish HPL Measurement Standards	2.8
	• Analysis and Design of Adaptive Phased Array	2.6
	• Analysis and Design of Phase-Locked Lasers	2.6
JPL	• Nuclear Laser Technical Support	2.6
	• Nuclear Laser Experimental Support	2.6

Table VII-3

ACTIVITIES OF LITTLE USEFULNESS TO NASA

--TERMINATION OF STUDY RECOMMENDED

Laboratory	Program	Value
Ames	• Arc-Heated GDL	1.2
	• Electronic Recombination Laser	2.2
	• High Brightness Laser Facility	0.6
	• Isotope Separation	1.2
Langley	• High Pressure CO_2 Tunable Laser	1.6
	• Atmospheric Transmission Studies	1.6
Lewis	• CO_2 Closed-Cycle CW Laser Scale-Up to Flight System	1.2
	• Beam Shaping for Maximum Power	2.4

range advancement. There are, however, noticeable differences in the talents, orientations and abilities of the staffs at the four laboratories. The key question is how to best utilize the limited amount of manpower that is available to the best possible advantage?

Although in some parlance the $6·million* that NASA is presently spending per annum on laser research might be regarded as a lot of money, it is actually quite small, both in terms of the grandoise long-range motives of the work and also in terms of the national commitment to laser research embodied in defense funding. To answer the question of how best to use NASA's limited resources, we must consider the constraints imposed by the available budget.

Referring back to Table I-1, it can be seen that NASA is presently supporting just under 100 man-years of effort per annum. Table VII-4 shows how the resources are apportioned:

Table VII-4

APPORTIONMENT OF NASA LASER RESOURCES

Laboratory	Funding/yr.	Man Years/yr.
Ames	$ 1.55 M	20
Lewis	2.31 M	37
Langley	1.48 M	21
JPL	1.14 M	20

The budget is spread thin, indeed, over a very large number of projects and a very large geographical area. This, naturally,

* As shown in Table I-1, this sum includes salaries, overhead functions, and Management Support as well as outside contracts.

makes it difficult to coordinate the work; and poor coordination leads to inefficient use of resources. One possible "fix" is to expand the budget for increased travel and coordination. This should make it possible not only for the various NASA researchers to keep abreast of their own mutual interests, but also for them to travel to other facilities of other agencies to better understand their programs and requirements. We have noted at several points in this report that NASA has insufficient knowledge of DoD's laser programs in spite of the fact that there are many cross-connects of NASA's programs with those efforts.

Another possible improvement would be to concentrate NASA's laser work at fewer laboratories. When an agency is involved in a low-budget effort, consolidation is often a good idea. It appears to us that it would be relatively easy to cut operations from four labs to three. It would also be very advantageous to concentrate all of the work on specific problems at one center. Hence, for example, *Photochemistry* might become a major research effort at one center only.

Finally, we wish to make an observation about <u>application orientation</u> versus <u>research orientation</u> as guiding principles for laser work at NASA's laboratories. Most of the programs that we recommend for termination (cf. Table VII-3) were not uninteresting, but simply irrelevant to NASA's needs. In most cases this could have been easily perceived, and wasted effort could have been avoided. On the other hand, there are programs which obviously have a great future even though <u>specific</u> applications may not be immediately apparent. *Laser Energy Conversion* is a good example of a technology area whose uses have not yet been fully defined. Surely there is some danger of such programs being "put on the shelf" just when they have succeeded, but this is not too likely in the case of really good ideas because applications tend to grow from "follow-ons" to the research. Good judgement usually permits recognition of good ideas; and, hence, there is still room for a lot of pure research sans immediate applications.

Projecting ahead toward the year 2000, it is certain that lasers will have a role in NASA's future. The extent of that role will depend largely upon NASA's ability to persuade the public that lasers are worth the needed investment to achieve full exploitation. If the public truly believes in the "L-5 Space Colony" concept*, laser propulsion may provide the only practical means for achieving that end. This is the ort of "driving inspiration" upon which NASA's researchers and decision makers can capitalize.

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*cf., July, 1976, National Geographic Magazine

APPENDICES

APPENDIX A

WORKING OUTLINE USED IN EVALUATING NASA CENTERS

NASA AMES RESEARCH CENTER

PHYSICAL GAS-DYNAMICS & LASERS BRANCH: (SPACE PHYSICS, ATMOSPHERIC PHYSICS, & SPACE FLIGHT TECHNOLOGY)

(A primary goal is new ideas. Hence there is a close collaboration with academia.)

HIGH POWERED LASERS RTOP, 506-25-41: (Primarily near term needs - device & technology orientation - Evaluation of needs for NASA missions)

I. GAS DYNAMIC LASERS

A. COMPUTER MODELS:

1. 1st Generation Code
   a. Nozzle ratio 10 to 20
   b. Stag. pres. 10 Atm.
   c. Stag. temp. 1000-1300°K

2. 2nd Generation Code
   a. Nozzle ratio 20 to 100
   b. Stag. pres. 10 to 200 Atm.
   c. Stag. temp. 1300-2500°K

This code allows parametric optimization of output power as a function of:
a. cavity geometry
b. mirror transmission & absorption
c. gas mixtures of CO_2, N_2 and H_2O.

B. SMALL CONTINUOUS FLOW ARC-HEATED FACILITY.
(To date has run at 25 atm. & 2200°K for 30 second periods. Modifications hopefully will produce 2nd generation conditions)

1. To experimentally verify computer studies
2. To study effects which cannot be calculated such as geometry & gas injection
3. To provide versatility not attained by others who use shock tube driver GDLs
4. To investigate water in the gas mix at high pressure
5. To investigate effects of other contaminants such as occur when air rather than pure nitrogen is used - various fuel combinations

II. CO SUPERSONIC EDL PROGRAM (CO SEDL)

(Potentially has twice the efficiency in closed cycle as CO_2 laser and half the wavelength)

A. COMPUTER MODEL RESULTS:

1. For efficiency, CO SEDL must utilize a non self-sustaining discharge photoionization source (E-beam)
2. Need alternate preionization sources for both NASA & DOD applications because they are: (a) cheaper, (b) simpler, (c) more reliable, (d) produce no x-rays
3. Best operating conditions for NASA missions are considerably different than AFWL missions

B. EXPERIMENTAL PROGRAM (Concentrates on alternate excitation methods)

1. Double Discharge Method
   a. Developed at Stanford under NASA contract. (Still under study in small CW laser supersonic wind tunnel)
   b. Will be studied at ARC in small "blowdown" tunnel designed to duplicate and extend the Stanford method.

2. Pulser-Sustainer Method (POKER)
   a. For this work, a large blowdown supersonic laser wind tunnel has been built. (AFWL & LRC-NASA are experimenting with POKER excitation for small CO_2 and CO lasers. ARC has only supersonic POKER. All AFWL funded programs (Boeing & Northrop MSNW) use E-beam. ARC has unique capability provided by burst-mode high voltage pulser & large capacitor bank for sustainer power supply.)

The goals of these programs (1&2) are to:

1. Demonstrate that both methods work in a CO SEDL.
2. Investigate limits of operation such as
   a. discharge energy loading
   b. control of average electron energy, etc.
3. Demonstrate simultaneous high power & efficient operation.
4. Evaluate feasibility of CO SEDL for potential NASA & AFWL missions.

III. LASER ENERGY CONVERSION

(Efforts to convert laser energy into electricity, shaft horsepower, or storable chemical energy. All OAST centers plus JPL have programs related to this problem)

A. CONFERENCES:

ARC organized, hosted & reported two conferences on laser energy conversion. (See Aeronautics and Astronautics, July-August, 1975)

B. STUDY CONTRACTS AWARDED BY ARC:

1. Rasor Associates - thermo-electric converter, (theory & small experiment).
2. Westinghouse Research - laser engines.
3. U.C. Berkeley - MOM optical diodes.
4. Princeton - laser dissociation of water.

ARC says the results of the various programs are encouraging. They want to go to detailed engineering designs & experimental tests. Big problem: Window materials; Sapphire is one of few known materials.

C. ARC IN-HOUSE THEORETICAL STUDIES:

1. Up-conversion from high power, high intensity IR to visible.
2. Stirling engine.

IV. THEORY: VIBRATIONAL ENERGY TRANSFER FROM DIATOMIC MOLECULES

(Details of excitation of specific vibrational states.) Collisional conversion of vibrational energy to translational energy has important effect on population of a particular vibrational state. Very little quantitative information exists on V-T rate dependence on quantum number, even for simplest diatomic molecules.

QUANTUM ELECTRONICS RTOP, 506-25-32
(ADDRESSES FUNDAMENTAL QUESTIONS WHICH, ALTHOUGH "HIGH RISK" MAY HAVE LONG-TERM PAYOFFS)

EXAMPLE OF SUCCESSFUL EFFORT: Complete theoretical description of the absorption of all of the important laser lines by laboratory plasmas. This greatly contributes to: (1) Laser fusion studies, (2) Laser communication through plasma sheaths, (3) Diagnostic measurements of plasma temperature and electron densities, (4) Concept of direct laser-electric energy conversion in thermionic diodes. (This ARC concept led to contractor development of the TELEC device.)

I. ELECTRONIC RECOMBINATION LASER

GOALS: Attempt to make GDL for $\lambda < 2.5\mu$. Try to produce population inversion in lower electronic states of an ionically recombing atomic vapor by collisional quenching from admixed molecules without external energy input. Measure relevant quenching cross-sections experimentally.

EFFORTS: (A) Numerical integration of Na-N_2-Ar through gentle nozzle to predict populations. (Key uncertainty affecting this possibility is quenching cross-sections).

(B) Small experiment using heated Na vapor pumped by 3303 Å dye laser in presence of N_2 will measure fluorescence decay as a function of N_2 pressure.

(C) These results may be extended to other atomic & molecular species since quenching by molecular species is very common & does not depend on exact energy level resonances.

II. HIGH BRIGHTNESS LASER FACILITY

GOALS: Use high brightness Nd: Glass laser to study laser-matter interactions. Investigate possible inversion (at λ=117Å) in aluminum plasma. Investigate inversion of soft x-ray transition levels produced by charge exchange collisions between highly ionized plasma (produced by laser) and an ambient background gas.

EFFORTS: Drive Nd: Glass laser with mode-locked Nd: YAG oscillator equipped with pulse selector & two amplifiers. Try to achieve 10^{12} Watt pulses with pulse widths adjustable from 25 p.sec. to 1 n.sec. Use intensity profiling & Faraday isolators to avoid self-damage to laser by self-focusing or back-reflections from targets.

III. LASER ISOTOPE SEPARATION

GOALS: Separate isotopes of a variety of molecules, including BCl^3, SF^6, OsO^4, SiF^4 and H^2O/D^2O. (Laser selectively excites the desired isotopic species. Another laser or other mechanism then ionizes the excited species to implement separation).

EFFORTS: (A) Use high power (>1 GW/Cm^2) pulse from CO_2 laser to excite vibrational transitions of isotopic molecule.

(B) Attempt to use 10.6μ CO_2 lines which overlap D_2O lines to separate heavy water from H_2O.

IV. HARMONIC CONVERSION OF IR LASER WAVELENGTHS

This important concept originated at ARC. They have maintained leadership in the field.

GOALS: Develop efficient up-conversion of high intensity/high efficiency IR lasers (such as CO & CO_2) to short wavelengths. This would (1) significantly reduce the mass & dimensions of transmitting & receiving optics and (2) couple energy better to detectors.

PROBLEM: Some recent progress has been made on the up-conversion problem by using two photon resonances in metal vapors. But, for IR input photons, species having level spacings of only a few tenths of an eV can be used, viz., molecular vapors. This is difficult because of (1) their spectral complexity, (2) unknown oscillator strengths, and (3) very low oscillator strengths among the vibrational levels in the ground electronic state (much smaller than for atoms).

KEY DISCOVERY: Virtual vibronic (vibrational-electronic) transitions can be used. Since such transitions are primarily electronic, they have much larger matrix elements than two-photon vibrational ground state transitions. Because of this, efficient conversion can be achieved with molecular systems.

EFFORTS: (A) Preliminary Study: H_2 (only molecule with completely documented oscillator strengths) was used to generate theoretically the 3rd harmonic of λ = 4.81μ.

(Without assuming special provisions, such as phase matching, the calculated conversion efficiency (ratio of output third harmonic to input pump) was very encouraging. For the plane wave case (no focusing of beam into hydrogen cell, an efficiency of 10% was achieved for only 1.8 KW.)

(B) Will use tunable lasers to measure other oscillator

(C) Will use calculated values using wave functions of Arnold et al to establish a wider choice of molecular candidates (such as CO & CH^3F which are already known to have two-photon resonances with CO_2 laser radiation.

(D) Will attempt experimental confirmations of promising candidates.

V. THEORY

THEORY: Calculations of the effects of collisions of photons, electrons, atoms and molecules with other atoms or molecules.

GOALS: (1) To guide the development of gas lasers.
(2) To predict interactions of photons with gases & plasmas.
(3) To contribute to laser induced chemistry experiments.

EFFORTS: (A) Codes for computing cross sections for rotational & vibrational excitation in collisions of atoms with diatomic molecules.

(B) An analytical expressions has been developed for the coefficient of absorption of photons by neutral atoms.

(C) Absorption coefficients of the inert gases have been computed. Coefficients for other elements can be readily computed.

(D) The coefficient for absorption of laser radiation by a hydrogen plasma has been calculated. The theory compares favorably with experiments.

(E) These laser-matter interaction studies are being extended to include multi-photon effects. This should lead to highly accurate two-photon photoionization cross sections.

VI. TUNABLE LASER APPLICATIONS

TUNABLE LASER APPLICATIONS: ARC has established a lab with three tunable lasers operating respectively, in the ranges 0.25 to 2.5μ, 0.25 to 0.75μ and 0.35 to 0.65μ. A computer interfaced to a waveform digitizer provides state-of-the-art data acquisition. APPLICATIONS INCLUDE:

(A) Measurements of cross sections & rate constants.
(B) Measurements of oscillator strengths.
(C) Assessment of quality of computed wave functions.
(D) Assessment of isotope separation for NASA needs, such as:
  (1) Space shuttle materials
  (2) Improved efficiency & cost reduction of RTGs.
(E) Assessment of laser induced chemistry for NASA needs, such as:
  (1) Measurements of rate constants for Shuttle pollution.
(F) Measurement of spectral response of laser energy converters.
(G) Attempt to induce coherent radiation at UV and x-ray wavelengths by harmonically pumping plasma oscillations in metals.

LASER AND MOLECULAR PHYSICS BRANCH

RESEARCH AREAS (RTOP 506-25-43)

I. HIGH PRESSURE CO_2 TUNABLE LASER CONCEPT FOR IMPROVING ATMOSPHERIC PROPAGATION:

(Pressure broadening of 10.6μ CO_2 lines amounts to ~ 3 GHz/Atm. Hence, at altitudes above 2.5 Km only 2 Atm. of cavity pressure allows sufficient tuning off of atmospheric CO_2 absorption line to increase transmission from 50% to 90%).

This idea originated at Langley. The immediate effort involves CO_2. Other molecular species are also of interest.

A. ISSUES

1. The alternative of using isotope lasers is not practical.
2. Trade-off studies are needed to compare advantages of tuning off CO_2 absorption line versus advantyes of kinetic cooling effect.
3. High pressure also implies high power density, compact devices; hence, there may be advantages for airborne & spaceborne applications.

B. IDEAS

1. High pressure lasers may be preionized by methods other than E-beams, such as: (1) UV flashlamps, (2) low ionization-potential seed material (organic gases, e.g. trimethylamine), (3) nuclear particles. Such methods would permit uniform lasing media of large volume. Thus extremely high power tunable lasers with high electric discharge efficiency (>20%) & good frequency stability may be feasible.

Direct nuclear excitation of high pressure CO_2 lasers has not been demonstrated. CO has been demonstrated, however. - - Any form of preionization would benefit large volume devices.

2. Proper preionization techniques could also lead to extreme frequency stability (< MHz) medium power (~ 1 KW) high pressure lasers.

Such devices would be well suited for "photon missions" such as:
1. Radar mapping of detailed surface features of planets or other distant space objects from earth orbiting satellites.
2. Remote sensing of atmospheric components of other planets or the earth itself.
3. Optical radar for missile defense.

C. EFFORTS

1. CO_2 high pressure laser (built by Javan under contract) is being used to map pressure broadened line profiles. The laser operates at 3 Atm. with an energy of ~ 0.1 J/pulse. The grating & telescope provide ~ 3GHz resolution.

Present data is very rough. They know that they need better wavelength resolution & line identification to make significant measurements of pressure broadening & tuning.

2. Another contract with Javan will seek to investigate:
a. Instabilities of off-peak lines while maintaining high efficiencies.
b. Travelling wave (instead of standing wave) configurations to reduce the number of longitudinal modes.
c. Mode locking to avoid fluctuations between longitudinal modes.
d. Use of high pressure CO_2 absorption cells inside the cavity to force operation off natural (1 Atm) frequency.
e. Effect of excess pressure (to 10 Atm.) on tuning & stability.
f. Conditions for higher pulse rates & energies.
g. Trade-offs between highly stable & reproducable pulsed operation.
h. Propagation in lab and in field.
i. Oscillator - amplifier possibilities.
j. Trade-offs between tuning off lines & kinetic cooling.
k. Comparative studies of various preionization techniques.
l. Comparative laser design requirements for key NASA & DOD missions.

It is not clear from the position paper how the work will be divided between Javan's group & the Langley group. Nor is it clear what work will be done theoretically, computationally, or experimentally. Taken at face value, the projected program is supposed to single-handedly solve practically all problems of NASA and DOD.

II. ATMOSPHERIC TRANSMISSION STUDIES

For the purpose of augmenting data inputs to existing NASA & DOD propagation computer codes, high resolution spectroscopic studies have been conducted in two modes:

A. Long path laboratory experiments have been made. They have agreed favorably with existing theoretical estimates. Present & upcoming activities include:

1. Use of tunable diode lasers to measure the 9.6μ band of ozone (O_3) with high resolution.

This will be very relevant to surface-to-space propagation of 9.28μ CO_2 radiation now being investigated by AFWL.

2. Use of tunable diode lasers to measure propagation at wavelengths corresponding to pressure-broadened wings of CO and DF laser lines.

This measurement, relevant to high pressure CO & DF laser development, will complement propagation measurements now being done at AFWL with fixed-frequency CO & DF lasers.

B. High resolution atmospheric transmission measurements are being made, using a tunable laser heterodyne spectrometer to observe the sun. This will address the problem of uncertainties in the aerosol attenuation at various altitudes, particularly at DF wavelengths.

This effort exactly duplicates the work of Hartwick's group at The Aerospace Corporation. Perhaps it is justified, however, by the difficult & variable nature of the absorption problem (i.e. two measurements may be better than one).

C. Langley also has considerable expertise in determining aerosol distribution by LIDAR ranging. This could be applied to the DF problem.

III. RESEARCH ON HIGH ENERGY MOLECULAR LASERS

Most or all of this work is being done by Cool at Cornell under contract with Langley.

A. CHEMICAL LASER INSTABILITIES

Study of vibrational-rotational (V → R) rotational-rotational (R → R) relaxations in chemical lasers. This multiline cascading results in improperly controlled output. Controlled multiline or single line operation cannot be achieved until the problem is understood.

EFFORT

Use multiwavelength excitation and fluorescence detection of the resulting transmissions to obtain key data.

It is not clear what level of effort is involved, or what technology is available at Langley.

B. CHEMICAL LASING IN THE VISIBLE

1. Experiment using rapid preheating of metal compounds in a shock tube, supersonic mixing with oxidizer, and rapid initiation of chemical reactions with an E-beam.

2. Experiment using dissociation of metal compounds with electric discharges - - Shows promise for high energy molecular laser concepts.

C. EXCIMER RESEARCH

1. Studies of KrF. Great potential exists for exciting KrF with high energy photons or nuclear particles.

HIGH POWER LASER PROGRAM

OBJECTIVE: "TO DEFINE THE POTENTIAL OF HIGH POWER LASERS FOR FUTURE NASA MISSIONS."

STRATEGY : (1) UNDERSTAND RELEVANCE OF HELS TO NASA POTENTIAL NEEDS.
(2) EVALUATE CRITICAL TECHNOLOGY.
(3) ASSESS POTENTIAL APPLICATIONS.

The program is touted as broader than DOD or ERDA efforts, which may address single objectives such as a particular weapon system, laser fusion, radar, or isotope separation. The Lewis program is supposed to address NASA applications in a flexible fashion such that laser technology is pushed ahead on a broad front. An effort has been made to avoid duplication of the work of other agencies, and a joint effort to establish measurement standards for HELs has been undertaken with DOD and NBS.

KEY PREMISE: NASA applications will require long duration operations. This implies:
(1) Closed-loop operation to conserve lasant, and
(2) High average power CW to avoid high peak intensities.

RESEARCH AREAS:

I. CO_2 TECHNOLOGY Closed-loop CW EDL has been built. Presently 5 - 10KW using ballasted multiple pin discharge excitation. Can be scaled to 70KW using an electron beam for ionization. Objectives:

A. FLEXIBLE FACILITY for investigation of:

1. Scaling laws (ultimately to 10 MW)
2. Operational characteristics
3. Contamination and long life effects
4. Resonator/optics designs

B. SOURCE OF RADIATION FOR OTHER NASA APPLICATIONS RESEARCH. May permit:

1. Comparison with non-laser techniques
2. Studies of airborne/spaceborne tradeoffs

Rather grandiose claims are made about uniqueness of approach & difference from ERDA, DOD, etc. E.g."The results of these studies should serve as a starting point for any possible future detailed design of specific closed-cycle EDL or GDL laser systems & be of benefit to DOD & ERDA as well as NASA. This, of course, overlooks possibilities of new technology, such as solar energized lasers.

II. TRANSMISSION AND PROPAGATION Active programs are underway to investigate large, light weight mirrors for space applications and adaptive optic techniques to compensate for beam distortions introduced by the atmosphere, including beam channel heating, turbulence, & scattering:

A. CONCEPTUAL DESIGN STUDY OF 30m DIFFRACTION LIMITED SPACE MIRROR HAS BEEN COMPLETED. PURPOSE:

1. Ascertain feasibility of deployment

a. This study assumed EVA
b. Other promising concepts include:
(1) inflatable membranes
(2) alloys with "memories" (e.g. Nitinol)

2. Develop concepts for:
a. Construction
b. Storage
c. Surface control

This section makes many naive statements which seem to reflect little appreciation for the state of progress of DOD work. It appears that they are about to repeat much work already completed.

The study reaches the conclusion that no technology is required beyond state-of-the-art, even in areas of sensing and control logic.

B. APPLICATION OF ADAPTIVE TECHNIQUES TO HIGH-POWER LASER SYSTEMS. Covers:

1. Effect of atmosphere on CO_2 HEL beams
2. Implications of high-power for system components
3. Conceptual design of a system capable of beaming large amounts of power from the ground to a satellite.

III. VISIBLE LASERS - PROGRAM SEEKS TO DEVELOP:

1. Smaller optics
2. More effective long range transmission
3. Efficient conversion of laser beams to electrical power
4. CW operation at visible wavelengths

Again there seems to be much duplication of efforts being pursued elsewhere.

ELECTRICALLY EXCITED EXCIMER LASER PROGRAMS INCLUDE:

A. FUNDAMENTAL SPECTROSCOPY & MOLECULAR KINETICS OF TWO PROMISING MOLECULES:

(1) Cs - Ar
(2) Na - Xe

B. EFFORT TO DEMONSTRATE CW LASER USING:
(1) XeF
(2) KXe
(3) Hg_2

IV. APPLICATIONS - EXPLORATORY STUDIES, ..."IN CONTRAST TO IN-DEPTH STUDIES." CURRENT STUDIES INCLUDE:

Applications seems to be the strong suit at Lewis Research Center.

A. LASER PROPULSION - Unique advantage is the potential to generate very high propellant temperatures (exceeding 10,000°K). Efforts address:

1. Thruster problems:
a. Laser propellant interactions
b. Laser energy absorption
c. Plasma formation & maintenance
d. Stability
e. Energy transfer & loss mechanisms in the flowing gas plasma medium

2. Types of missions
3. Possible trajectories
4. Characteristics of both laser transmitter & receiver systems

USAF Rocket Propulsion Lab has recently initiated a systems study to explore & define the feasibility of laser propulsion missions for DOD. Lewis personnel participated in the RFP definition & in proposal review.

B. LOW WEIGHT ON-BOARD POWER CONVERSION SYSTEMS FOR SATELLITES Lewis has assumed the role of defining systems. They will:

1. Implement studies & experimental programs to:
a. Define system parameters
b. Evaluate various concepts
c. Inter-compare alternate systems

C. MATERIALS PROCESSING WITH LASERS (Investigating both ground based & spaced based possibilities)

1. Exploring potential of high power CO_2 holographic techniques.
2. Early consideration of zero gravity, high vacuum possibilities.

APPENDIX B

A REVIEW OF THE NASA/AMES ARC-HEATED GASDYNAMIC LASER PERFORMANCE

WJSA-TM-76-01

Prepared by: Dr. G. W. Zeiders

A REVIEW OF THE NASA/AMES ARC-HEATED GASDYNAMIC LASER PERFORMANCE

Prepared by: G. W. Zeiders

14 January 1976

Prepared for:

NATIONAL AERONAUTICS AND SPACE ADMINSTRATION

Contract No.: NASW-2866

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TABLE OF CONTENTS

ILLUSTRATIONS

1.0 INTRODUCTION

Gasdynamic lasers operate by virtue of the population inversion which can be produced between vibrational energy states when a gas is rapidly expanded. If the relaxation rate of the upper laser level is slow compared to that of the lower, the first will temporarily be characterized by a temperature approaching that of the plenum, whereas the other will tend to thermalize at the actual gas temperature (lower due to the conversion to directed kinetic energy).

If nearly pure nitrogen pumps a small quantity of CO_2, the maximum possible available laser energy in the flow is given by

$$P_A/\dot{m} = 185\left(e^{\frac{3353}{T_N}} - 1\right)^{-1} \text{ Kj/lb}$$

where T_N is the effective vibrational temperature of the excited N_2. As shown below, there is a dramatic improvement to be gained by "freezing" at the highest possible value of T_N:

T_N, ^oK:	1000	1500	2000	2500	3000
$P_A/\dot{m}$, Kj/lb:	6.70	22.2	42.6	65.6	89.9

NASA Ames Research Laboratory has chosen to take advantage of this by heating the working gas to temperatures as high as 3000^{o}K in a Marquardt arc unit. Since combustion is not involved, the approach offers a wide degree of flexibility in gas composition.

In view of questions regarding the kinetics and the ability to extract power from the flow, W. J. Schafer Associates, Inc. has been asked to critique the concept. Reviewed in this report are: 1) vibrational deactivation kinetics, 2) laser gain, 3) species dissociation.

2.0 VIBRATIONAL DEACTIVATION KINETICS

The high-temperature operating capability of the arc heater, together with the possibility of high-pressure operation (up to 200 atm.) with area ratio 26 and 40 nozzles, raises concern about vibrational deactivation: i.e., can the favorable available power described in Section 1 be realized by effective freezing in the nozzles, and can it be maintained in the supersonic cavity flow?

The primary rates of importance to the $N_2/CO_2/H_2O$ gasdynamic laser kinetics (shown schematically in Figure 1) are presented in Figures 2-4. There is obviously considerable scatter between the data of different investigators (primarily at high temperatures), and the indicated correlations represent best estimates based on consideration of expected trends, the experimental techniques used, and the experimentalists. As shown in Appendix B-1, the rates of loss of available power via collisional deactivation of excited nitrogen and carbon dioxide, respectively, are proportional to $k_{NML}\psi_M$ and $\psi_{CO_2}/\left[k_p^{-1} + (\Sigma\psi_M k_{3ML})^{-1}\right]$ where ψ_M is the concentration of the collision partner. For typical concentrations of interest, i.e., $0.01 < \psi_{CO_2} < 0.20$ and $0.005 < \psi_{H_2O} < 0.05$ with $\psi_{H_2O} \leq \psi_{CO_2}$, direct deactivation of N_2 by H_2O is generally small compared to CO_2 deactivation by H_2O or N_2; deactivation of excited N_2 by other partners is even less. Consequently, the characteristic length for available power loss by deactivation, i.e., that for which e-folding can occur, is essentially

$$\ell = \frac{u}{\psi_{CO_2}}\left[\frac{1}{k_p} + \frac{1}{\Sigma\psi_M k_{3ML}}\right].$$

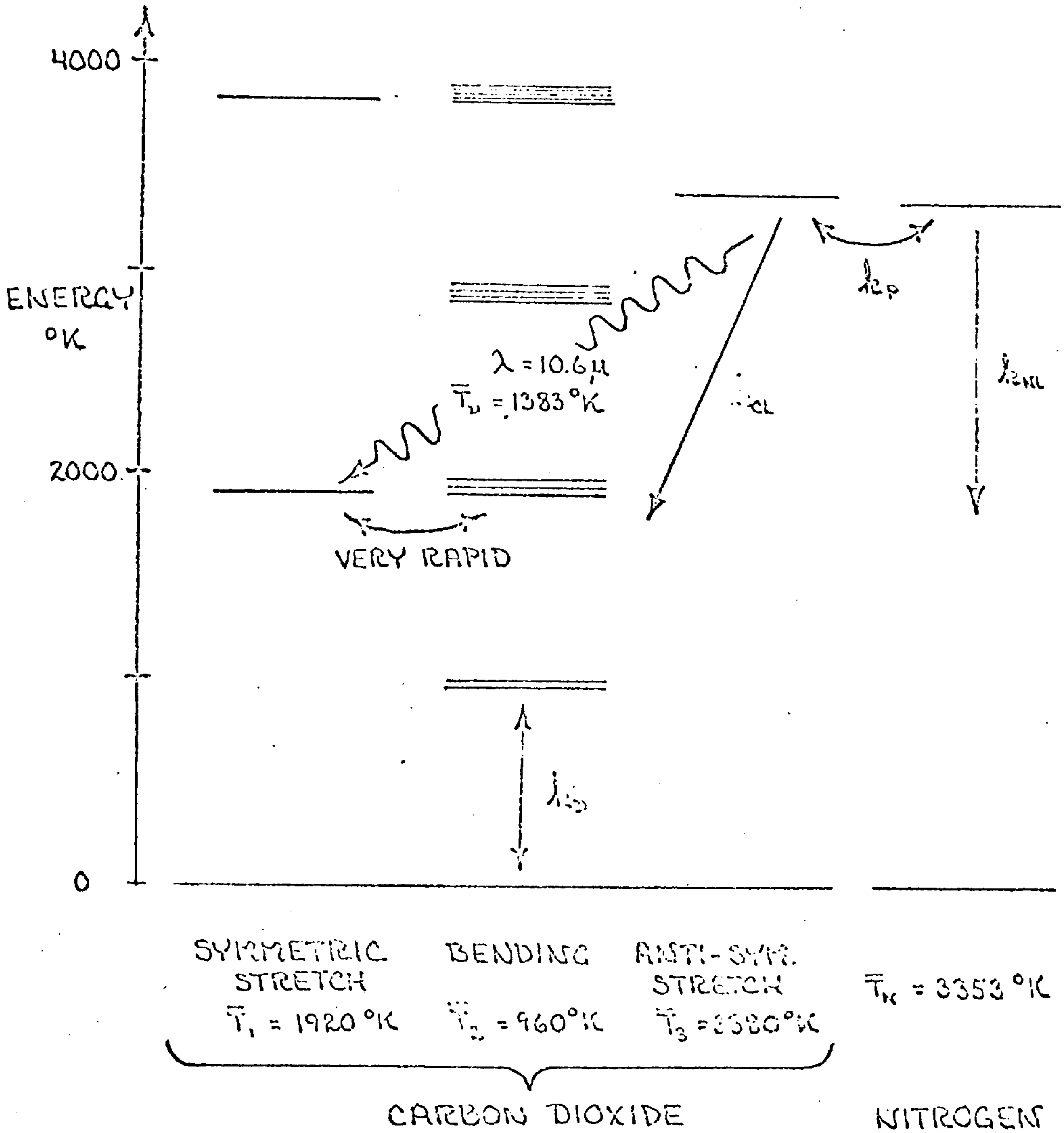

FIGURE 1 KINETIC MODEL

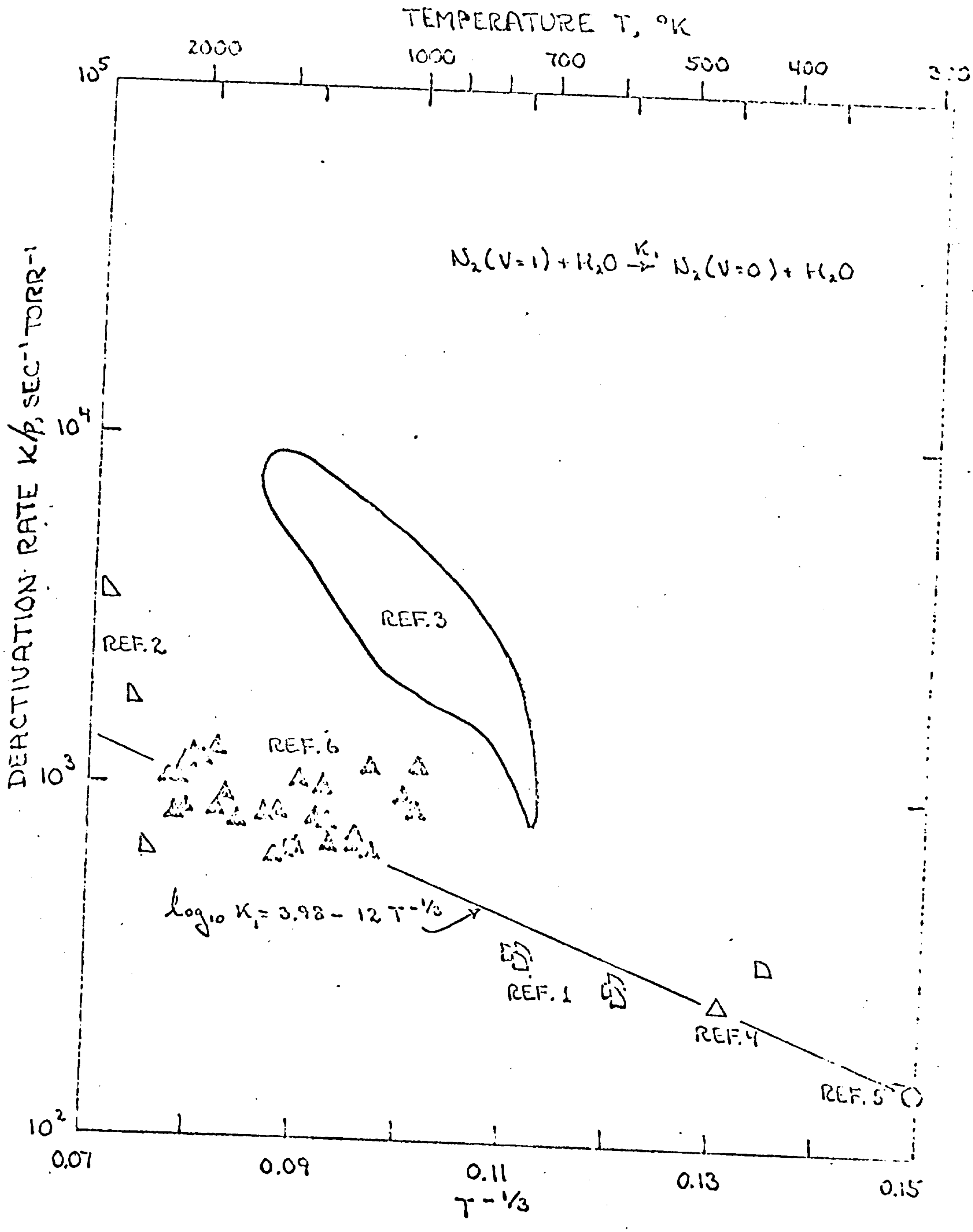

FIGURE 2 N_2^* DEACTIVATION

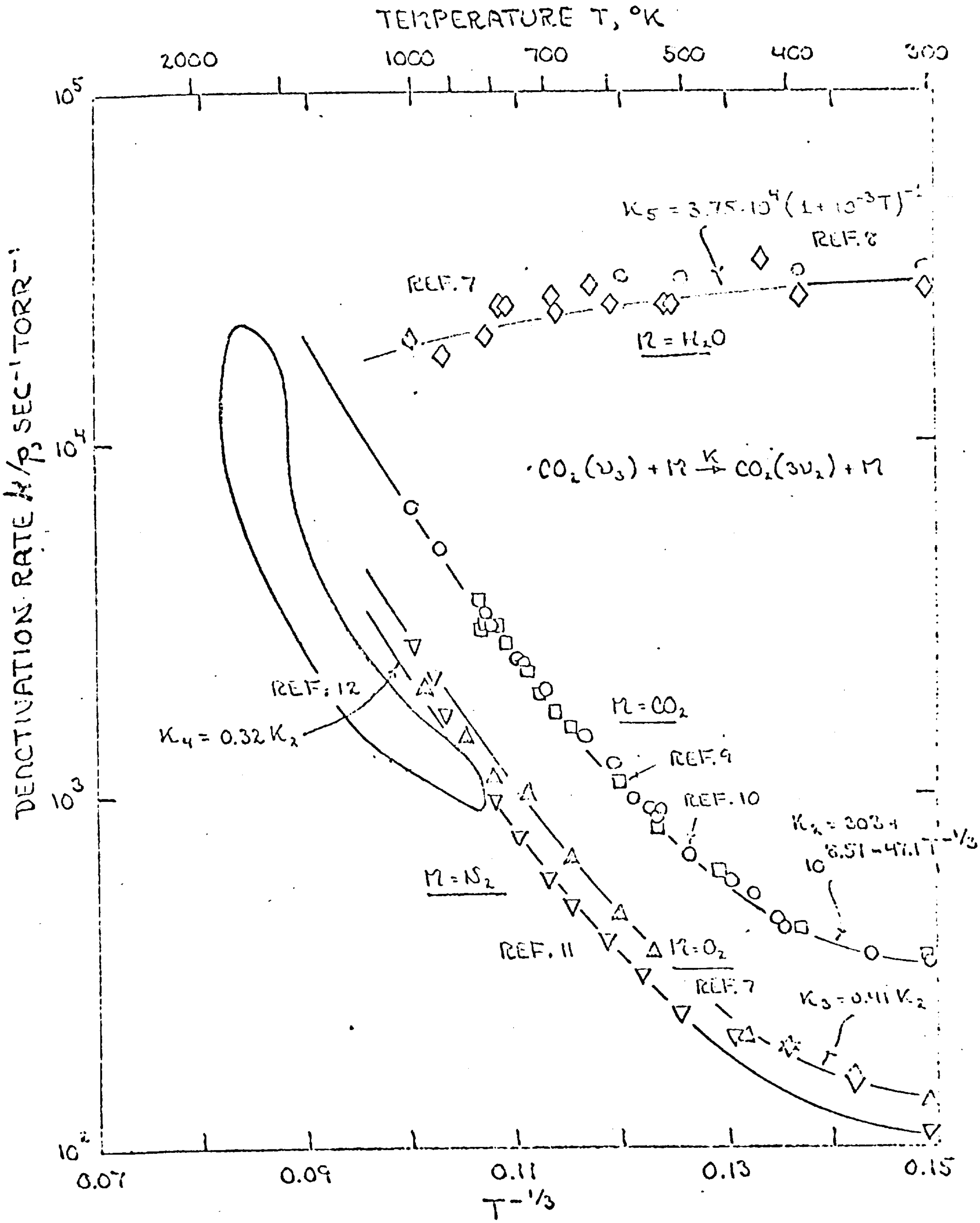

FIGURE 3 $CO_2(\nu_3)$ DEACTIVATION

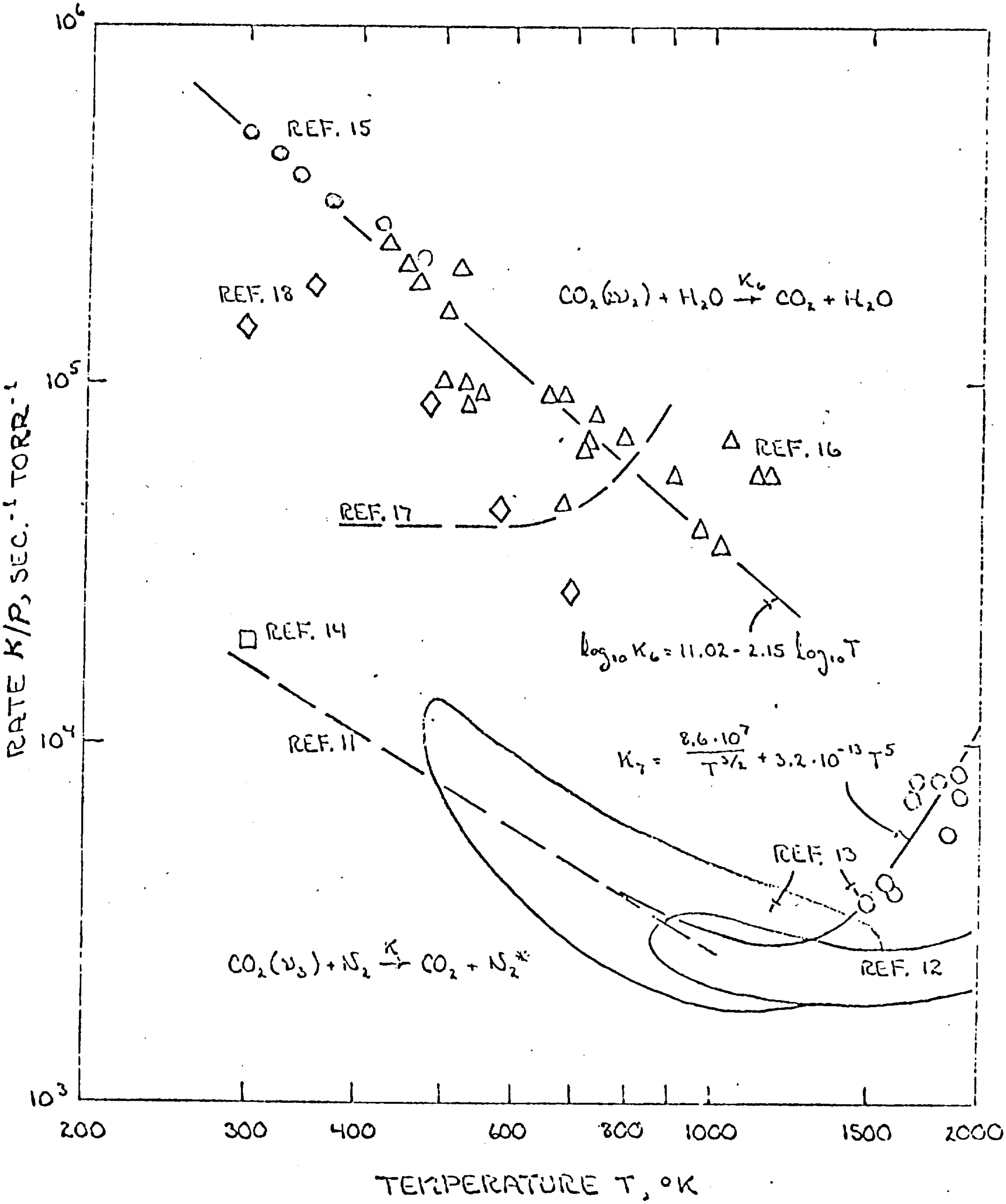

FIGURE 4 PUMPING AND $CO_2(\nu_2)$ DEACTIVATION

Pumping is extremely rapid at the temperatures characteristic of cavity operation and provides no barrier to deactivation there, i.e., the excited CO_2 and N_2 remain very closely coupled, but it can be an important source of gain saturation with strong laser power extraction. However, it can impede deactivation of CO_2 at high temperatures typical of the "freezing" zone.

It is convenient to represent the overall deactivation length ℓ by

$$\ell = \ell_p' + \left(\frac{1}{\ell_{3N}} + \frac{1}{\ell_{3H}}\right)^{-1}$$

where ℓ_p' , the effective pumping length, ℓ_{3N}, the CO_2 deactivation length by N_2, and ℓ_{3H}, the CO_2 deactivation length by H_2O, are plotted in Figure 5 as a function of area ratio for plenum temperatures of 2000^oK and 3000^oK. The curves for $\gamma = 1.3$ correspond essentially to equilibrium (fully-relaxing) expansion, while $\gamma = 1.4$ corresponds to frozen flow. Consequently, a transition from the vicinity of the former towards the latter can be expected in the vicinity of the nozzle throat ($h/h^* = 1$) with a well-configured design. Also shown in Figure 5 is the available 10.6μ laser power for nearly pure N_2 at equilibrium (i.e., for $T_N = T$); higher available power will result, of course, beyond the freezing point.

In the vicinity of the sonic throat, deactivation by H_2O at typical concentrations will always be negligible, and loss due to CO_2 deactivation by N_2 will be the dominant effect; however, if the rates can be trusted at high temperature, it can be seen from Figure 5 that the pumping rate (as noted previously) will retard the loss somewhat. The effective

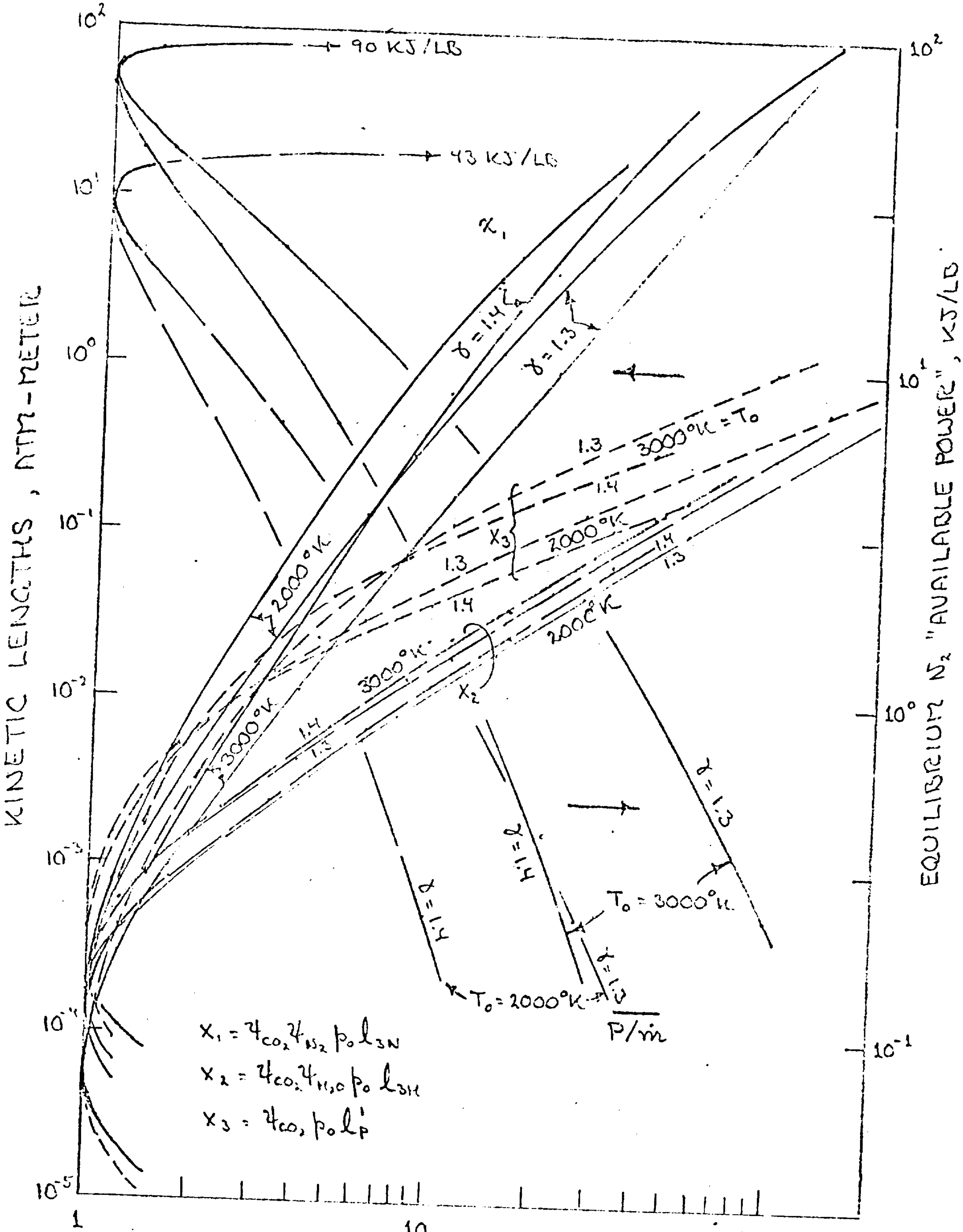

FIGURE 5 CHARACTERISTIC LENGTHS

deactivation length at the throat at $T_o = 2000^oK$ is about five times that at 3000^oK, but the extremely rapid rise with distance (much of the expansion occuring within one throat height beyond the throat) will diminish the effect of the difference. In view of the equilibrium available energy benefit of the higher temperature, it appears certain that improved frozen energy will always result at the highest possible temperatures attainable with the NASA arc heater if $\psi_{CO_2} p_o h^*$ is less than about 2 atm-mm. Since increased CO_2 partial pressure will always delay the transition (reducing freezing efficiency), and since the cavity volumetric benefit of increased pressure will generally be offset from an overall systems viewpoint by the increased mass flow requirement, NASA should restrict plenum pressure to the lowest value compatible with the exhaust system unless the experiments are specifically devoted to deduction of effective rates.

Within the optical cavity, pumping will always be very rapid compared to collisional deactivation. More water can be tolerated at lower area ratios and higher plenum temperatures, but its helpful effect on the lower CO_2 laser state (Figure 4) decreases with increasing cavity temperature. Collisional deactivation within the cavity should not be a serious problem with reasonable pressure and concentrations. Experiments devoted to rate determination at cavity conditions are not warranted in view of the excellent data already existing there.

Since $T_o = 3000^oK$ offers no more than about a factor of two in maximum available power over $T_o = 2000^oK$, serious consideration should be given to materials problems at the higher temperature. Other detrimental effects of increased temperature are considered in the remainder of this report.

3.0 GAIN COEFFICIENT

The gain coefficient is the measure of the amplification of the laser medium, and, as such, it determines the ability to extract useful energy. In the case of an oscillator, it must be sufficient to overcome all losses (including transmission): with an unstable resonator with 75% output coupling, a reasonable limiting guideline for good beam quality, the product of the gain coefficient G and the total (two-way) path length L must exceed about 1.4 for oscillation to be sustained.

The gain coefficient for a CO_2 laser operating on a P-branch transition is given by

$$G = \beta \frac{\psi CO_2}{Q} \frac{\bar{T}_v}{T} \left[e^{-\frac{\bar{T}_3}{T_3}} - e^{-2(J+1)\frac{\bar{T}_R}{T}} e^{-\frac{\bar{T}_3 - \bar{T}_v}{T_1}} \right]$$

where $$Q = \left[1 - e^{-\bar{T}_3/T_3}\right]^{-1} \left[1 - e^{-\bar{T}_2/T_2}\right]^{-2} \left[1 - e^{-\bar{T}_1/T_1}\right]^{-1}$$

is the vibrational partition function, and where $J = P-1$ is the rotational quantum number. The parameter β is essentially independent of pressure for $p > 0.05$ (i.e., where pressure broadening dominates Doppler effects) and is weakly dependent upon concentrations for a typical $N_2/CO_2/H_2O$ gasdynamic laser. Its principle dependence is

$$\beta \cong 0.60 \sqrt{\frac{\bar{T}_R}{T}} (2J+1) e^{-J(J+1)\frac{\bar{T}_R}{T}} \text{ cm}^{-1}$$

which is shown in Figure 6 to be virtually constant at 0.51 cm^{-1} for operation on the optimum rotational transition, a condition often selected by a resonator (it is interesting to note, however, that minor gain variations result over a wide temperature range, giving rise to the line

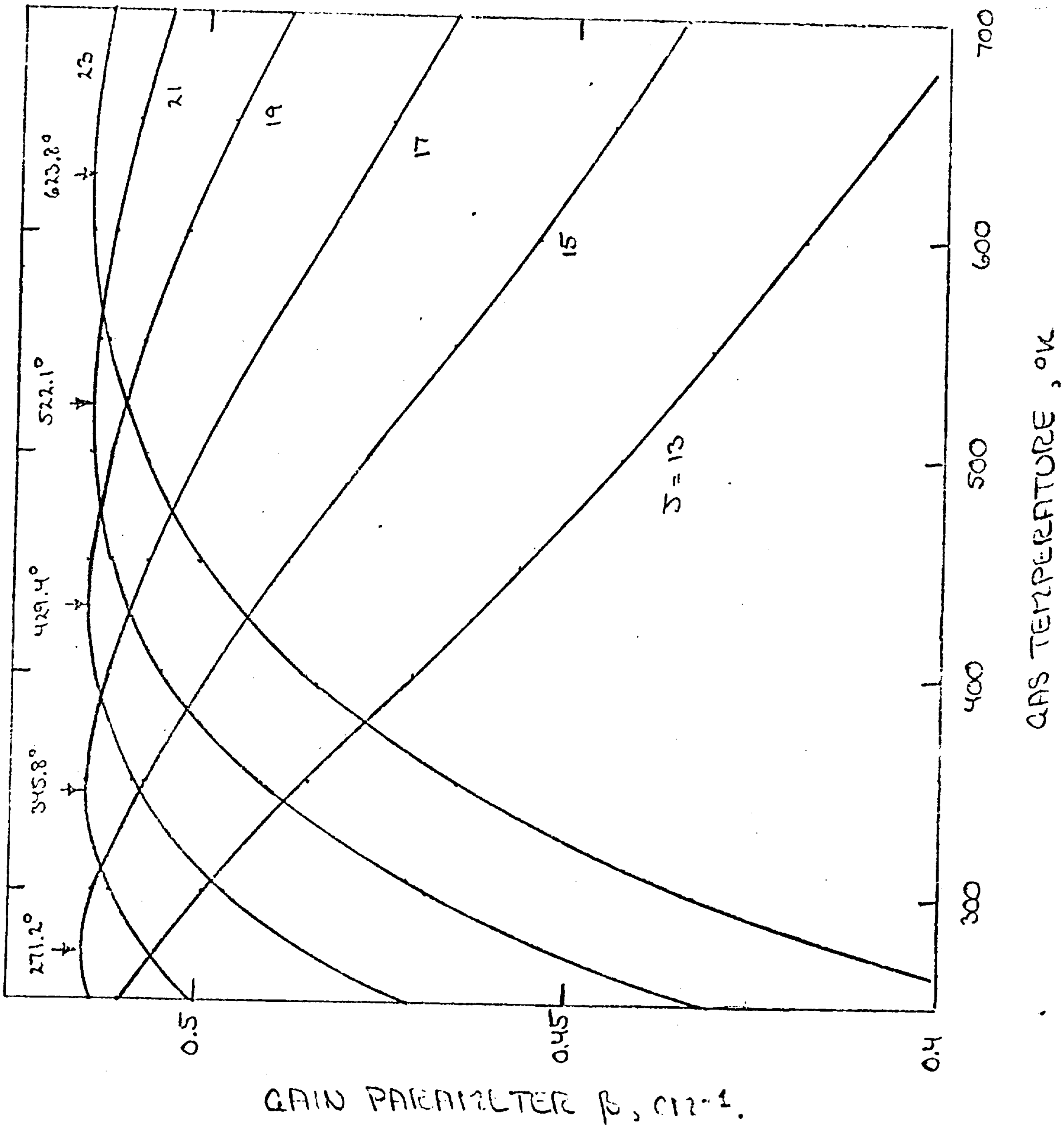

FIGURE 6 ROTATIONAL LINE DEPENDENCE

switching so often noted in CO_2 lasers). Using the peak β, the quantity G/ψ_{CO_2} is shown in Figure 7 for $T_2 = T_1 = T$, i.e., for very rapid deexcitation of the bending and symmetic stretch modes of CO_2. The rapid loss of gain at high gas temperature is due to filling of the lower vibrational states and to wide distribution through the rotational ones.

Superimposed on the gain curves are gasdynamic expansion ones for freezing at the nozzle throat. As noted previously, the upper limit for a given area ratio ($\gamma = 1.4$) corresponds to essentially frozen expansion beyond the sonic region, whereas the lower ($\gamma = 1.3$) is representative of equilibrium flow. Area ratio tends to have a much greater influence on gain than does T_3, the gain actually displaying a relatively flat maximum with T_3 with the peak occuring at less than $2000^{o}K$ for area ratios of interest. Since available power continues to rise with T_3, the actual optimum from an overall performance viewpoint will lie somewhat above the gain maximum.

The NASA arc heater experiments will permit gain investigations in this essentially-unexplored optimum region where rate data is sparse (Section 2), but a note of caution is advised regarding the J-dependence. Although an internal oscillator will tend to select the optimum transition, an external probe laser may vary over a range of rotational lines, and fairly significant errors can be incurred with off-optimum line operation. The variation of β shown in Figure 6 must be accounted for in the data reduction, and a spectrometer should always be used.

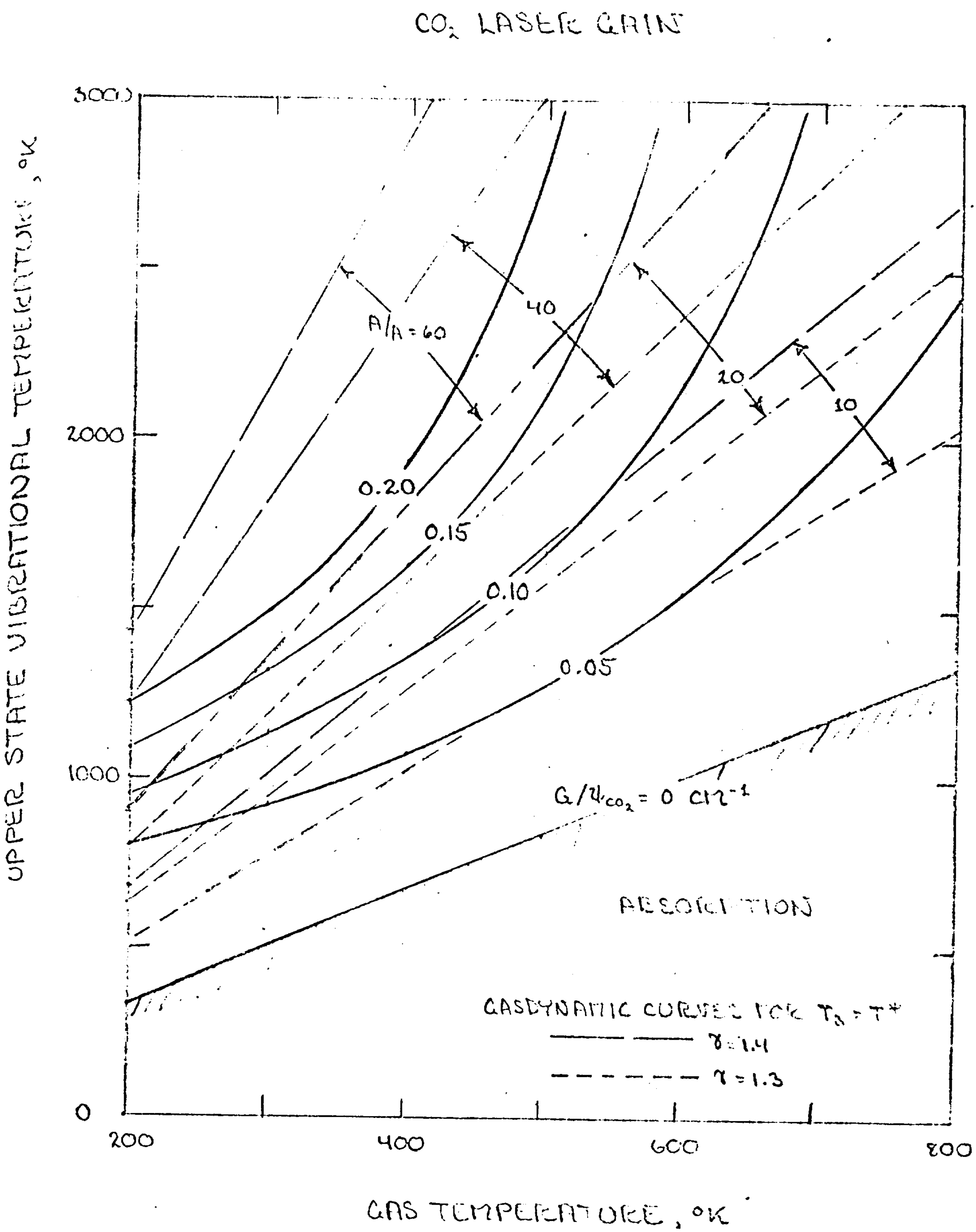

FIGURE 7 GAIN MAP

4.0 DISSOCIATION

The high temperatures which can be attained with the arc heater (up to 3000°K) can produce dissociation of the major species. Like the vibrational energy which "freezes" in the rapid expansion, the gas composition may also remain relatively unchanged from that in the plenum. To determine the extent of dissociation, equilibrium gas conditions were calculated for interesting mixtures from 1800-3000°K and from 10-100 atm. The calculations are presented in Appendix B-3, and representative results are shown in Figures 8-12. In general, the dissociation is negligible (less than 10% departure from the reference composition) for T < 2400°K, but CO_2 dissociation, in particular, becomes significant at higher temperatures. It is interesting to note that water tends to retard the formation of CO at the expense of increased O_2 and NO, neither of which have much effect on the system.

Due to the uncertainty regarding recombination rates, it is fortunate that a sonic throat temperature of 2400°K corresponds to a plenum temperature near or beyond the capability of the NASA arc heater, so dissociation should not affect the experiments. It is expected that equilibrium flow will exist in the subsonic region.

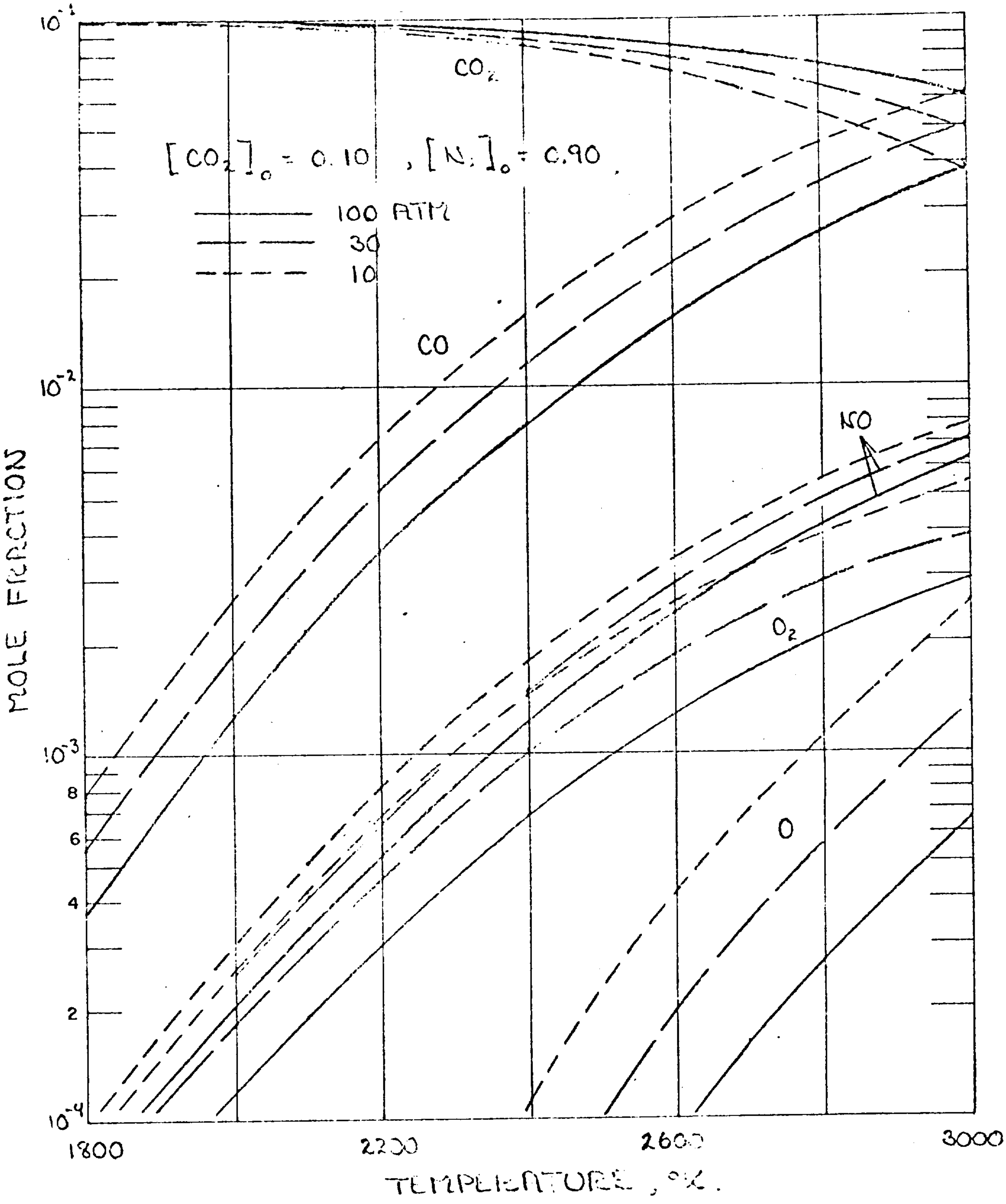

FIGURE 8 N_2/CO_2 DISSOCIATION

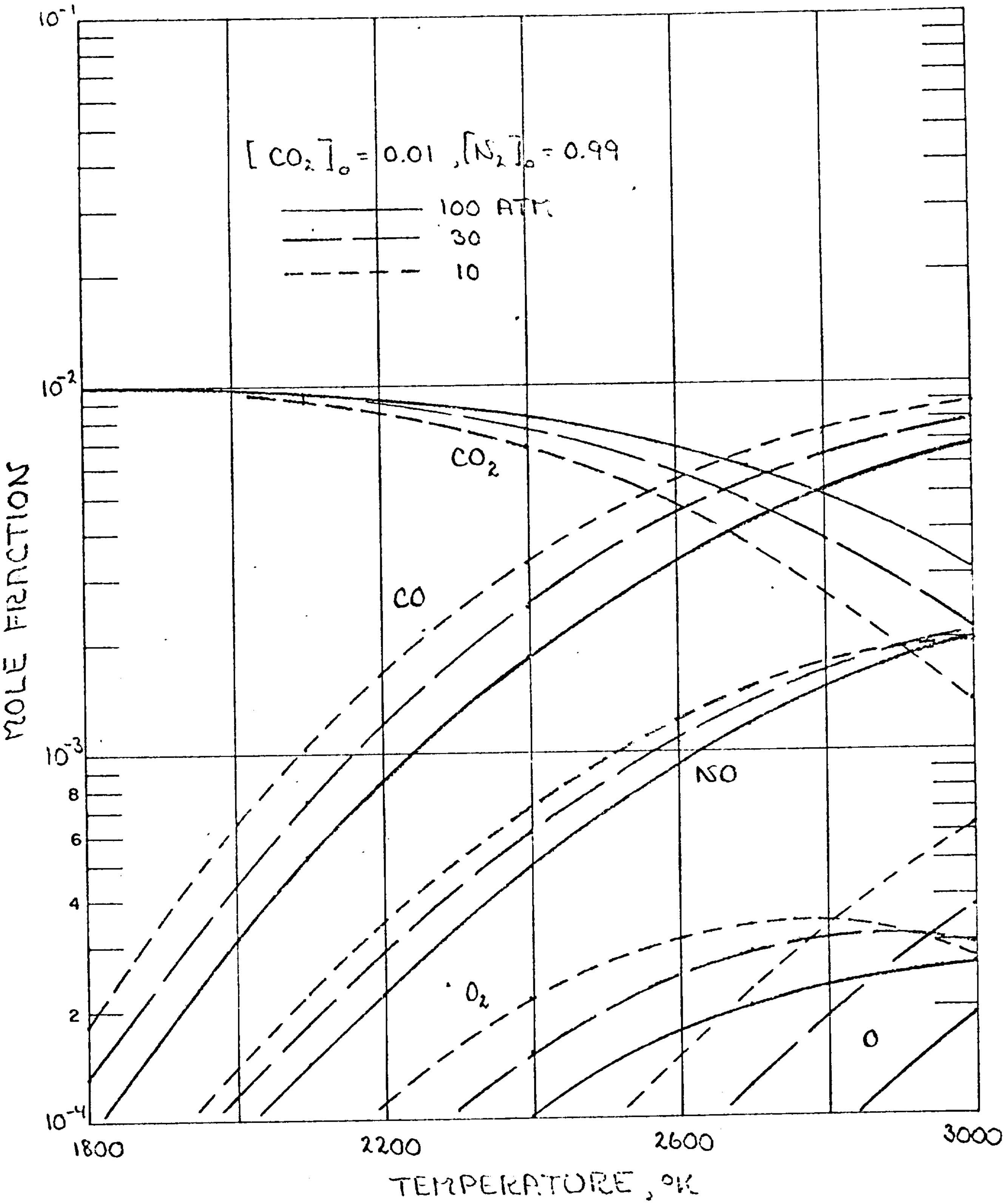

FIGURE 9 N_2CO_2 DISSOCIATION

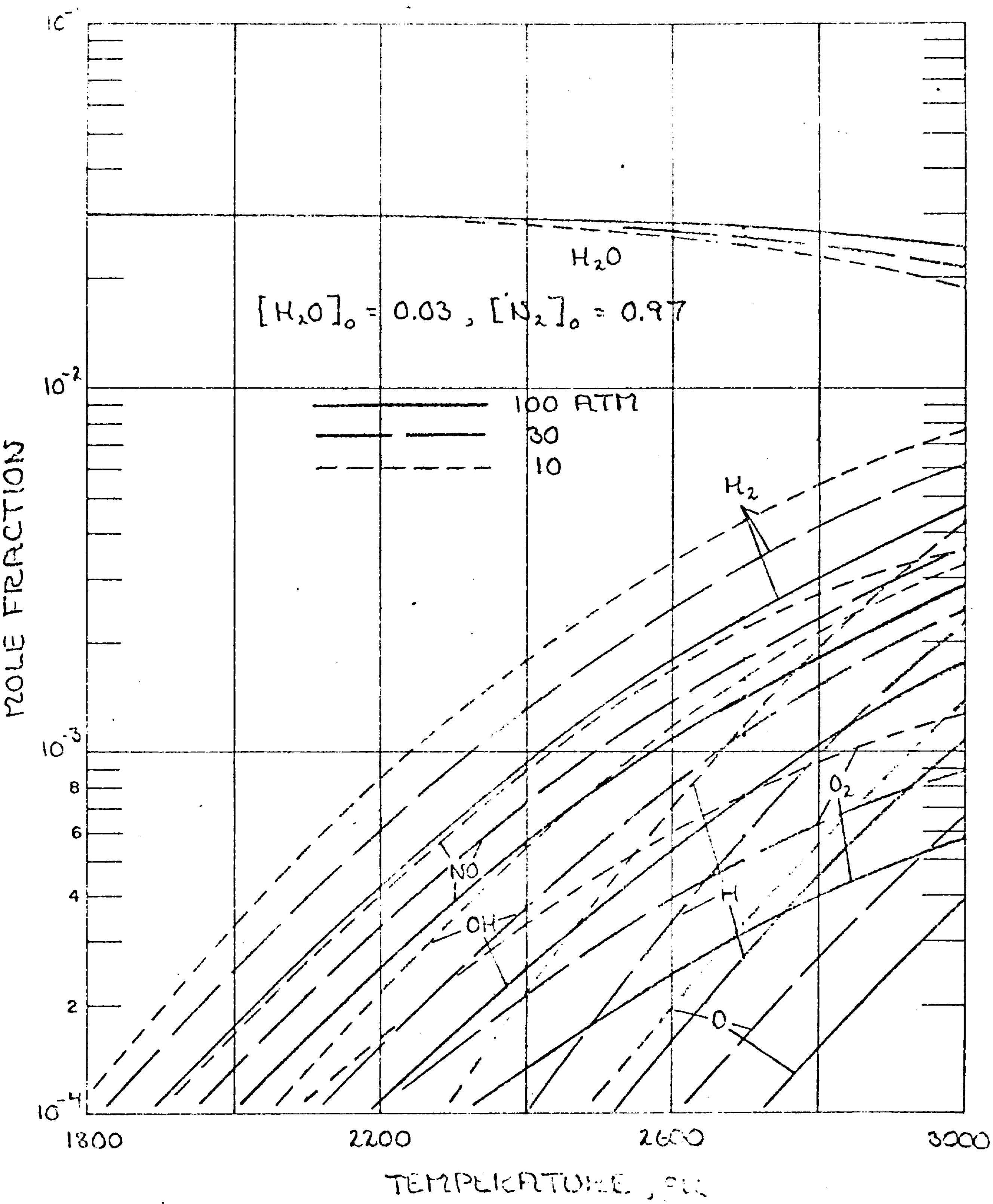

FIGURE 10 N_2/H_2O DISSOCIATION

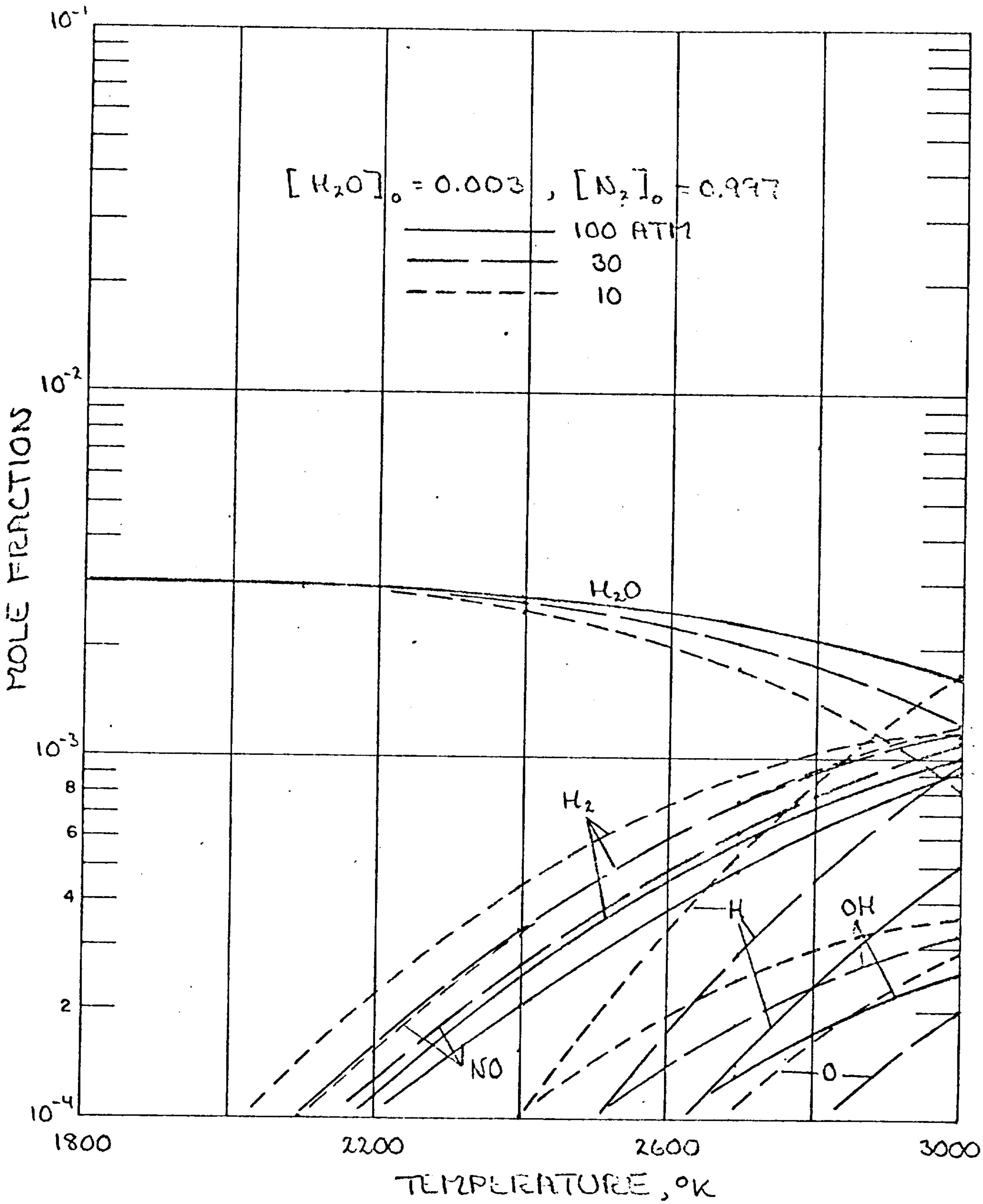

FIGURE 11 N_2/H_2O DISSOCIATION

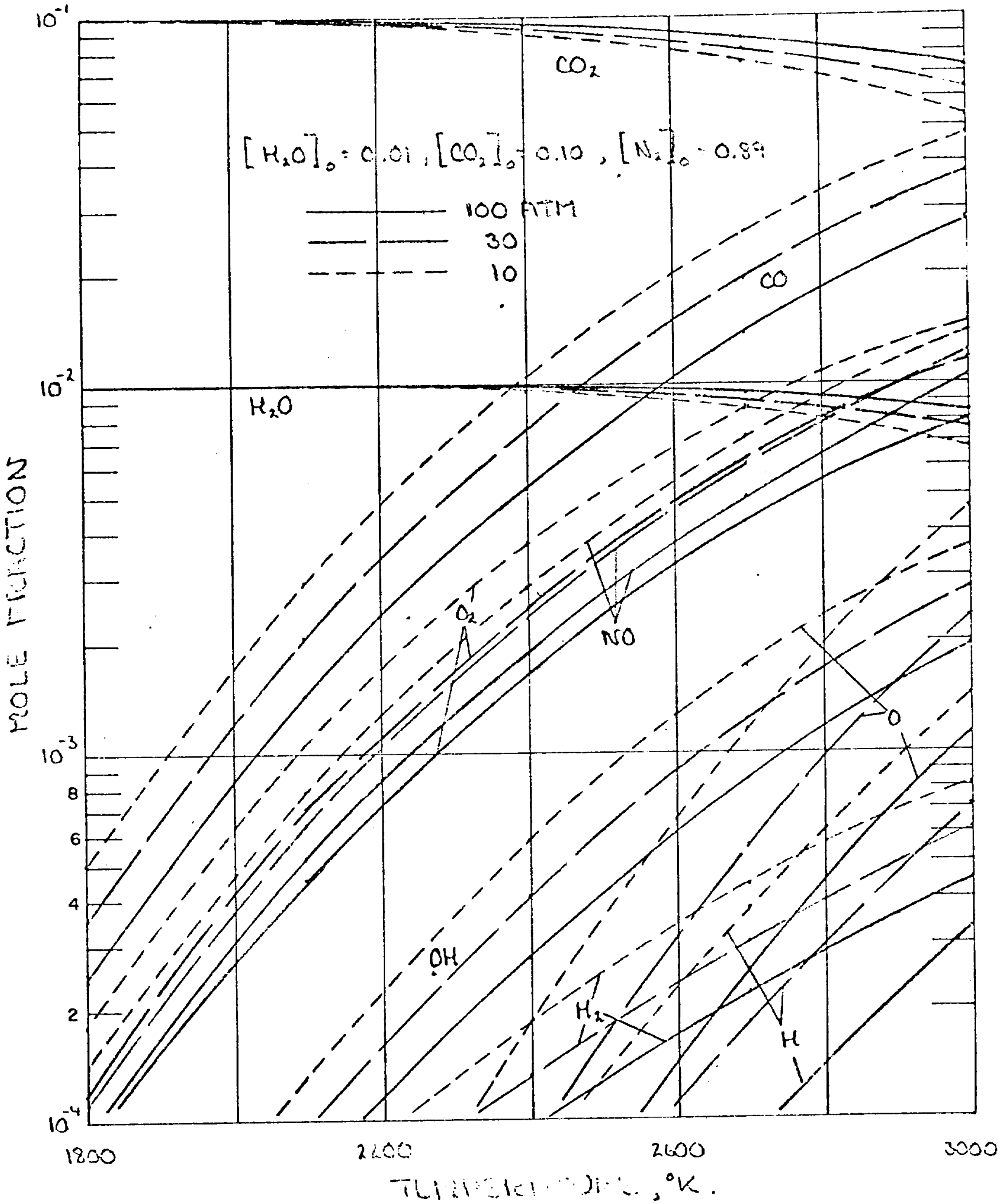

FIGURE 12 $N_2/CO_2/H_2O$ DISSOCIATION

5.0 CONCLUSIONS

The performance to be expected from the NASA arc-heated gasdynamic laser has been studied from the aspects of:

1) vibrational deactivation kinetics,
2) laser gain,
3) dissociation.

On the basis of these results, the following conclusions can be drawn:

1) good vibrational freezing can be expected if $P_o \psi_{CO_2} h^* \lesssim 2$ atm-mm for temperatures up to the system limit;
2) high-pressure operation (in excess of the above) is warranted only if high-temperature relaxation rates are to be inferred;
3) high-temperature rates have not been adequately determined, and the experiments may be justified on that basis alone, since optimum CO_2 laser performance may result with $T_o > 2000^\circ K$;
4) probe laser rotational line operation should be monitored with a spectrometer, and appropriate data corrections should be made;
5) dissociation/recombination kinetics may pose a major source of uncertainty for $T_o \gtrsim 2400^\circ K$.

Although the analyses suggest peak laser performance at high temperature, the advantage will be relatively small, and the rapid deterioration of material properties may offset the benefit. This factor should be explored before major costs are expended on the planned arc-heater experiments.

In view of the limited advantages of the high-temperature operation, the program is recommended only if its goal is the measurement of previously unexplored rate data. Even then, there are probably better techniques available.

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APPENDIX B-1

DEACTIVATION KINETICS

The vibrational energy per molecule is given by

$$E_i = \frac{g_i \overline{T}_i \psi_i}{e^{\overline{T}_i/T_i} - 1}$$

where g_i = degeneracy , $\overline{T}_i$ = characteristic temperature

ψ_i = species mole fraction, T_i = vibrational temperature .

The kinetics of the N_2/CO_2 system are described in part by

$$u \frac{dE_n}{dx} = \psi_{N_2} k_p \frac{E_3 E_n}{\theta_3 \overline{E}_n} \frac{\overline{T}_n}{\overline{T}_3} \left(e^{\frac{\overline{T}_n}{T_n} - \frac{\overline{T}_n}{T}} - e^{\frac{\overline{T}_3}{T_3} - \frac{\overline{T}_3}{T}} \right) - \sum \psi_m k_{nml} (E_n - \overline{E}_n)$$

and

$$u \frac{dE_3}{dx} = \psi_{N_2} k_p \frac{E_3 E_n}{\theta_3 \overline{E}_n} \left(e^{\frac{\overline{T}_3}{T_3} - \frac{\overline{T}_3}{T}} - e^{\frac{\overline{T}_n}{T_n} - \frac{\overline{T}_n}{T}} \right) - a\phi \frac{T}{p} \frac{\overline{T}_3}{\overline{T}_\nu}$$

$$+ \sum \psi_m k_{3ml} \frac{E_3}{\theta_3} \left(\frac{E_2}{\overline{E}_2} \right)^3 \left[e^{\frac{\overline{T}_3}{T_3} - \frac{\overline{T}_3}{T}} - e^{3\left(\frac{\overline{T}_2}{T_2} - \frac{\overline{T}_2}{T}\right)} \right]$$

where k_p = $N_2 \rightarrow CO_2$ pumping rate constant,

k_{ijl} = deactivation rate constant for state i by specie j,

T = gas temperature , a = laser gain coefficient

p = gas pressure , ϕ = local radiant flux

u = flow velocity , $\theta_i = 1 - e^{-\overline{T}_i/T}$, $\overline{E}_i = E_i(T_i = T)$.

Assuming that enough catalyst (H_2O in the NASA experiments) is available to deactivate the symmetric stretch and bending modes to $T_1 \cong T_2 \cong T$, neglecting the minor $27^\circ K$ deficit between $\bar{T}_n$ and $\bar{T}_3$, and assuming that $\exp(\bar{T}_i/T_i) >> 1$, the equations reduce to the simple linear set

$$\frac{dE_n}{dx} = \frac{1}{\ell_p}(\psi_{N_2}E_3 - \psi_{CO_2}E_n) - \frac{1}{\ell_n}(\bar{E}_n - \bar{E}_n)$$

and

$$\frac{dE_3}{dx} = \frac{1}{\ell_p}(\psi_{CO_2}E_n - \psi_{N_2}E_3) - \frac{1}{\ell_c}(E_3 - \bar{E}_3)$$

for no power extraction. The characteristic lengths are given by

$$\ell_c = \frac{u}{\Sigma \psi_m k_{3m1}} , \quad \ell_n = \frac{u}{\Sigma \psi_m k_{nm1}} , \quad \ell_p = \frac{u}{k_p} .$$

Combining the equations to eliminate E_3, we obtain

$$\frac{d}{dx}\left(\ell_p \frac{dE_n}{dx}\right) + \left(\frac{\psi_{CO_2} + \psi_{N_2}}{\ell_p} + \frac{1}{\ell_c}\right)\ell_p \frac{dE_n}{dx} + \frac{\psi_{CO_2}}{\ell_c}(E_n - \bar{E}_n)$$

$$+ \frac{d}{dx}\left[\frac{\ell_p}{\ell_n}(E_n - E_n)\right] + \left(\frac{\psi_{N_2}}{\ell_p} + \frac{1}{\ell_n}\right)\frac{\ell_p}{\ell_n}(E_n - \bar{E}_n) = 0$$

For most cases of interest, the second derivative produces a very rapid initial transient which occurs over a length scale of about $\left(\frac{1}{\ell_p} + \frac{1}{\ell_c} + \frac{1}{\ell_n}\right)^{-1}$. Over longer distances, the term can be ignored, resulting in the approximate solution

$$E_n \cong \int_0^x \bar{E}_n \, e^{-\int_s^x \frac{ds^1}{\ell}} \frac{ds}{\ell} \quad , \quad \ell \cong \frac{\ell_c + \ell_p}{\psi_{CO_2}}$$

when direct deactivation of the excited nitrogen is neglected.

In the nozzle of a gasdynamic laser, the pressure and temperature fall rapidly as the flow is accelerated. Consequently, $\bar{E}_n$ decreases very rapidly, essentially vanishing at high Mach numbers, while ℓ, which was zero in the plenum, rises dramatically. The net effect is to cause the integrand to have a very pronounced maximum at a point within the nozzle, generally near the sonic throat, from which most of the contribution to the integral occurs. Thus, the available energy in the laser cavity can be represented by

$$E_n \cong E_{nf} \, e^{-\frac{x}{\ell_{cav}}}$$

where the "freezing value" E_{nf} is primarily a function of the properties near the aforementioned maximum, and ℓ_{cav} is dependent on the properties in the cavity. Scaling dimensions with the throat height h^*, and noting that ℓ_c is dominated by nitrogen ($\psi_{N_2} \cong 1$) at elevated temperatures, E_{nf} is primarily dependent upon $p_o \psi_{CO_2} h^*$ and T_o for a given nozzle contour. Furthermore, if h^*/ℓ^* is less than about unity, it is primarily the subsonic and sonic contours which determine E_{nf}.

The effective deactivation length can be represented by

$$\ell = \frac{1}{1/\ell_{3n} + 1/\ell_{3m}} + \ell_p^1$$

where $x_1 = \psi_{CO_2}\psi_{N_2}P_o\ell_{3n} = \dfrac{0.100(T_o/T)^{\frac{\gamma}{\gamma-1}}\sqrt{\frac{\gamma}{\gamma-1}\,(T_o-T)}}{303 + 10^{8.51 - 47.1T^{-1/3}}}$ atm-m

$$x_2 = \psi_{CO_2}\psi_{H_2O}P_o\ell_{3m} = 8.55\cdot 10^{-7}\left(\frac{T_o}{T}\right)^{\frac{\gamma}{\gamma-1}}\sqrt{\frac{\gamma}{\gamma-1}(T_o-T)}\,(1+10^{-3}T) \quad \text{atm-m}$$

$$x_3 = \psi_{CO_2}P_o\ell_p^{\,1} = 3.21\cdot 10^{-2}\,\frac{\left(\frac{T_o}{T}\right)^{\frac{\gamma}{\gamma-1}}\sqrt{\frac{\gamma}{\gamma-1}(T_o-T)}}{\frac{8.6\cdot 10^{7}}{T^{3/2}} + 3.2\cdot 10^{-3}T^{5}} \quad \text{atm-m}$$

for a constant γ (ratio of specific heats) expansion. The temperature is in turn related to the local nozzle area ratio h/h^* by

$$\frac{h}{h^*}\sqrt{\frac{2}{\gamma-1}\left(\frac{T_o}{T} - 1\right)} = \left(\frac{2}{\gamma+1}\,\frac{T_o}{T}\right)^{\frac{\gamma+1}{2(\gamma-1)}} .$$

Tabulations for these follow for $\gamma = 1.3$ (corresponding to essentially equilibrium expansion) and for $\gamma = 1.4$ (frozen flow) for $T_o = 2000^oK$ and 3000^oK:

$\gamma = 1.3$, $T_o = 2000^oK$:

T:	1700	1400	1100	800	600	500	400	300
x_1:	$1.98\cdot10^{-4}$	$1.18\cdot10^{-3}$	$9.09\cdot10^{-3}$	$1.26\cdot10^{-1}$	1.25	4.82	20.50	95.9
x_2:	$1.68\cdot10^{-4}$	$4.91\cdot10^{-4}$	$1.50\cdot10^{-3}$	$5.88\cdot10^{-3}$	$1.97\cdot10^{-2}$	$4.20\cdot10^{-2}$	$1.07\cdot10^{-1}$	$3.55\cdot10^{-1}$
x_3:	$4.04\cdot10^{-4}$	$2.27\cdot10^{-3}$	$9.24\cdot10^{-3}$	$3.12\cdot10^{-2}$	$7.79\cdot10^{-2}$	$1.35\cdot10^{-1}$	$2.64\cdot10^{-1}$	$6.14\cdot10^{-1}$
h/h^*:	1.006	1.359	2.48	6.21	14.99	26.59	54.17	137.1

$\gamma = 1.4$, $T_o = 2000^oK$:

T:	1700	1400	1100	800	600	500	400	300
x_1:	$1.55\cdot10^{-4}$	$7.89\cdot10^{-4}$	$4.97\cdot10^{-3}$	$5.28\cdot10^{-2}$	$4.13\cdot10^{-1}$	1.36	4.82	17.73
x_2:	$1.32\cdot10^{-4}$	$3.28\cdot10^{-4}$	$8.17\cdot10^{-4}$	$2.46\cdot10^{-3}$	$6.47\cdot10^{-3}$	$1.19\cdot10^{-2}$	$2.50\cdot10^{-2}$	$6.56\cdot10^{-2}$
x_3:	$3.17\cdot10^{-4}$	$1.52\cdot10^{-3}$	$5.05\cdot10^{-3}$	$1.31\cdot10^{-2}$	$2.57\cdot10^{-2}$	$3.84\cdot10^{-2}$	$6.20\cdot10^{-2}$	$1.14\cdot10^{-1}$
h/h^*:	1.003	1.153	1.720	3.302	6.275	9.563	16.18	32.2

$\gamma = 1.3$, $T_o = 3000^oK$:

T:	2600	2200	1800	1400	1000	800	600	500
x_1:	$6.35\cdot10^{-5}$	$2.91\cdot10^{-4}$	$1.51\cdot10^{-3}$	$1.12\cdot10^{-2}$	$1.64\cdot10^{-1}$	$9.89\cdot10^{-1}$	9.51	36.1
x_2:	$2.38\cdot10^{-4}$	$6.18\cdot10^{-4}$	$1.58\cdot10^{-3}$	$4.64\cdot10^{-3}$	$1.86\cdot10^{-2}$	$4.62\cdot10^{-2}$	$1.49\cdot10^{-1}$	$3.14\cdot10^{-1}$
x_3:	$6.40\cdot10^{-5}$	$4.17\cdot10^{-4}$	$2.94\cdot10^{-3}$	$2.15\cdot10^{-2}$	$1.14\cdot10^{-1}$	$2.45\cdot10^{-1}$	$5.91\cdot10^{-1}$	1.01
h/h^*:	1.000	1.234	1.967	3.937	10.81	21.68	54.17	97.45

$\gamma = 1.4$, $T_o = 3000^oK$:

T:	2600	2200	1800	1400	1000	800	600	400
x_1:	$5.07 \cdot 10^{-5}$	$2.02 \cdot 10^{-4}$	$8.85 \cdot 10^{-4}$	$5.32 \cdot 10^{-3}$	$5.92 \cdot 10^{-2}$	$2.95 \cdot 10^{-1}$	2.24	25.4
x_2:	$1.90 \cdot 10^{-4}$	$4.29 \cdot 10^{-4}$	$9.27 \cdot 10^{-4}$	$2.21 \cdot 10^{-3}$	$6.69 \cdot 10^{-3}$	$1.38 \cdot 10^{-2}$	$3.50 \cdot 10^{-2}$	$1.32 \cdot 10^{-1}$
x_3:	$5.11 \cdot 10^{-5}$	$2.89 \cdot 10^{-4}$	$1.73 \cdot 10^{-3}$	$1.02 \cdot 10^{-2}$	$4.10 \cdot 10^{-2}$	$7.31 \cdot 10^{-2}$	$1.39 \cdot 10^{-1}$	$3.26 \cdot 10^{-1}$
h/h^*:	1.014	1.088	1.467	2.382	4.941	8.230	16.18	42.83

SR-52 Program:

T_o STO 01	RCL01	3030	y^x	÷	)	HLT
Y STO 02	÷	=	5	(	=	
T STO 03	RCL03	2nd 1/x	÷	RCL02	x	
2nd LBL A	)	x	1 EE 11	+	(	
1	y^x	RCL04	+	1	(	
–	RCL04	=	(	)	RCL02	
RCL02	)	HLT	2.7 EE 9	=	–	
2nd 1/x	=	(	÷	y^x	1	
=	STO 04	RCL03	RCL03	(	)	
2nd 1/x	RCL03	÷	y^x	(	÷	
STO 04	$\sqrt[x]{y}$	1000	1.5	RCL02	2	
x	3	+	=	+	÷	
(	÷	1	2nd 1/x	1	(	
RCL01	47.1	)	x	)	RCL01	
–	=	x	RCL04	÷	÷	
RCL03	2nd 1/x	RCL04	=	(	RCL03	
)	–	x	HLT	RCL02	–	
=	9.51	8.55	RCL01	–	1	
2nd $\sqrt{x}$	=	EE +/-7	x	1	)	
x	+/-	=	2	)	)	
(	INV 2nd LOG	HLT	÷	÷	2nd $\sqrt{x}$	
(	+	RCL03	RCL03	2	=	

APPENDIX B-2

GAIN COEFFICIENT

I) $$\beta = 0.60\sqrt{\frac{\bar{T}_R}{T}}\,(2J+1)e^{-J(J+1)\frac{\bar{T}_R}{T}}\ \mathrm{cm}^{-1}\ ,\ \bar{T}_R = 0.565^{o}\mathrm{K}$$

Maximum @ $\frac{T}{\bar{T}_R} = 2J(J+1) \rightarrow \beta_{max} = \frac{0.36(2J+1)}{\sqrt{2J(J+1)}} \rightarrow 0.51$ for $J >> 1$

Transitions: $\beta(J) = \beta(J+2)$ @ $\frac{T}{\bar{T}_R} = \frac{2(2J+3)}{\ln\frac{2J+5}{2J+1}} \rightarrow \frac{1}{2}(2J+1)(2J+3)$

J:	13	15	17	19	21	23	25	27
T:	237	307	386	475	572	678	793	

@ Transition: $\beta \rightarrow 0.51\sqrt{\frac{2J+1}{2J+3}}\ e^{\frac{4J+3}{(2J+1)(2J+3)}} \rightarrow 0.51$ for $J >> 1$

II) $G/\psi_c =$

$$0.51\,\frac{1383}{T}\left(1 - e^{-\frac{3380}{T_3}}\right)\left(1 - e^{-\frac{960}{T}}\right)^2\left(1 - e^{-\frac{1920}{T}}\right)\left[e^{-\frac{3380}{T_3}} - e^{-\frac{1.13(J+1)+1997}{T}}\right]$$

G/ψ_c	T: 200	400	600	800
0	336	670	1002	1334
0.1	962	1350	2171	(3196 @ 700)
0.2	1212	1933	(2832 @ 500)	
0.05	801	1071	1549	2435
0.15	1093	1626	3322	

III) Gasdynamics ($\varepsilon = A/A^*$):

$$M_\varepsilon = \left[\frac{2}{\gamma+1}\left(1+\frac{\gamma-1}{2}M^2\right)\right]^{\frac{\gamma+1}{2(\gamma-1)}}, \quad \frac{T^*}{T} = \frac{2}{\gamma+1}\left(1+\frac{\gamma-1}{2}M^2\right)$$

	ε:	10	20	30	40	50	60
$\gamma = 1.4$	M:	3.993	4.726	5.231	5.609	5.914	6.171
	T*/T:	3.998	4.255	5.394	6.076	6.662	7.181
$\gamma = 1.3$	M:	3.582	4.207	4.586	4.864	5.084	5.267
	T*/T:	3.544	3.178	3.613	3.955	4.241	4.489

J	4	fLN	RCL1	1
LBLA	RCLI	÷	x	+
STO1	2	R/S	RCL2	x
2	x	.565	÷	RCL2
x	1	÷	CHS	f√
3	+	STO2	f^{-1}LN	÷
+	÷	RCL1	RCL1	.6
1.13	1	1.	2	x
x	+	+	x	RTN

G/ψ_{CO_2} → STO1	f^{-1}√	+	f^{-1}LN	2
T	÷	f√	+	÷
LBLA	1920	1	gLSTx	fLN
STO2	RCL2	−	1	CHS
RCL1	÷	2	+	g1/x
x	CHS	÷	STO3	3380
705	f^{-1}LN	1	f^{-1}√	x
÷	CHS	+	gx><y	RTN → T_3
960	1	1.13	4	
RCL2	+	x	x	
÷	÷	1997	−	
CHS	RCL2	+	f√	
f^{-1}LN	3.54	RCL2	RCL3	
1	x	÷	gx><y	
−	1	CHS	−	

SR-52 PROGRAM:

$$\beta = 0.60 \sqrt{\frac{0.565}{T}}\,(2J+1)e^{-J(J+1)\frac{0.565}{T}}\ \mathrm{cm}^{-1}$$

{J STO 01}	+/-
{T}	INV ℓnx
2nd LBLA	x
STO 02	((
2nd 1/x	2
x	x
.565	RCL 01)
=	+
STO 03	1
x	)
RCL 01	=
x	x
(	.6
RCL 01	x
+	RCL 03
1	2nd $\sqrt{\ }$
)	=
=	HLT

J:	13	15	17	19	21	23
T:						
250	0.510	0.514	0.500	0.471	0.432	0.385
300	0.499	0.514	0.512	0.496	0.469	0.433
350	0.485	0.507	0.515	0.509	0.492	0.465
400	0.471	0.498	0.512	0.514	0.505	0.486
450	0.457	0.488	0.507	0.515	0.512	0.500
500	0.443	0.477	0.500	0.512	0.515	0.508
550	0.431	0.466	0.492	0.508	0.514	0.513
600	0.419	0.455	0.483	0.502	0.512	0.515
650	0.408	0.445	0.475	0.496	0.509	0.515
700	0.397	0.435	0.466	0.489	0.505	0.513

APPENDIX B-3

HIGH-TEMPERATURE DISSOCIATION

$$CO_2 \rightarrow CO + \frac{1}{2}O_2 \qquad K_1 = \sqrt{p}\,\frac{\psi_{CO}\sqrt{\psi_{O_2}}}{\psi_{CO_2}} \qquad \psi_{CO_2} + \psi_{CO} = a$$

$$H_2O \rightarrow H_2 + \frac{1}{2}O_2 \qquad K_2 = \sqrt{p}\,\frac{\psi_{H_2}\sqrt{\psi_{O_2}}}{\psi_{H_2O}} \qquad 2\psi_{H_2O} + 2\psi_{H_2} + \psi_{OH} + \psi_H = b$$

$$H_2O \rightarrow OH + \frac{1}{2}H_2 \qquad K_3 = \sqrt{p}\,\frac{\psi_{OH}\sqrt{\psi_{H_2}}}{\psi_{H_2O}} \qquad \psi_{H_2O} + \psi_{NO} + 2\psi_{CO_2} + \psi_{CO} + 2\psi_{O_2} + \psi_O + \psi_{OH} = c$$

$$N_2 + O_2 \rightarrow 2NO \qquad K_4 = \frac{\psi_{NO}^2}{\psi_{N_2}\psi_{O_2}} \qquad 2\psi_{N_2} + \psi_{NO} = d$$

$$H_2 \rightarrow 2H \qquad K_5 = p\,\frac{\psi_H^2}{\psi_{H_2}} \qquad \psi_{CO_2} + \psi_{CO} + \psi_{H_2O} + \psi_{H_2} + \psi_{OH} + \psi_H + \psi_{NO} + \psi_{O_2} + \psi_O + \psi_{N_2} = 1$$

$$O_2 \rightarrow 2O \qquad K_6 = p\,\frac{\psi_O^2}{\psi_{O_2}}$$

Initial conditions: $\psi_{CO_2} = a$, $2\psi_{H_2O} = b$, $\psi_{H_2O} + 2\psi_{CO_2} = c = \frac{b}{2} + 2a$

$2\psi_{N_2} = d \rightarrow d = 2(1-a) - b$, $c = 2a + \frac{b}{2}$

T:	K_1	K_2	K_3	K_4	K_5	K_6
3000	.3417	$4.628\cdot10^{-2}$	$4.841\cdot10^{-2}$	$1.472\cdot10^{-2}$	$2.475\cdot10^{-2}$	$1.441\cdot10^{-2}$
2700	.1013	$1.490\cdot10^{-2}$	$1.312\cdot10^{-2}$	$6.592\cdot10^{-3}$	$3.207\cdot10^{-3}$	$1.487\cdot10^{-3}$
2400	$2.195\cdot10^{-2}$	$3.634\cdot10^{-3}$	$2.573\cdot10^{-3}$	$2.410\cdot10^{-3}$	$2.516\cdot10^{-4}$	$8.738\cdot10^{-5}$
2100	$3.035\cdot10^{-3}$	$5.954\cdot10^{-4}$	$3.178\cdot10^{-4}$	$6.595\cdot10^{-4}$	$9.658\cdot10^{-6}$	$2.299\cdot10^{-6}$
1800	$2.135\cdot10^{-4}$	$5.383\cdot10^{-5}$	$1.964\cdot10^{-5}$	$1.170\cdot10^{-4}$	$1.277\cdot10^{-7}$	$1.819\cdot10^{-8}$

$$\psi_O = \sqrt{K_6}\,\frac{\psi_{CO_2}K_1}{p\psi_{CO}}, \quad \psi_{O_2} = \frac{1}{p}\left(\frac{\psi_{CO_2}K_1}{\psi_{CO}}\right)^2, \quad \psi_{H_2} = \frac{K_2}{K_1}\,\frac{\psi_{CO}\psi_{H_2O}}{\psi_{CO_2}}, \quad \psi_H = \sqrt{\frac{K_5}{p}\,\frac{K_2}{K_1}\,\frac{\psi_{CO}\psi_{H_2O}}{\psi_{CO_2}}}$$

$$\psi_{NO} = \sqrt{\frac{K_4}{p}\,\psi_{N_2}}\;\frac{\psi_{CO_2}K_1}{\psi_{CO}}, \quad \psi_{OH} = K_3\sqrt{\frac{K_1}{K_2}\,\frac{\psi_{CO_2}\psi_{H_2O}}{p\psi_{CO}}}$$

Note that dissociation is inhibited by high pressure.

$$\text{For } b = 0: \quad \psi_{CO} = \frac{2}{p}\left(\frac{\psi_{CO_2}}{\psi_{CO}} K_1\right)^2 + K_1\left(\sqrt{\frac{K_4}{p}(1-a)} + \frac{\sqrt{K_6}}{p}\right)\frac{\psi_{CO_2}}{\psi_{CO}}$$

$$\therefore \sqrt{p} = \frac{K_1 \frac{\psi_{CO_2}}{\psi_{CO}} \sqrt{K_4(1-a)} + \sqrt{K_4(1-a)\left(K_1 \frac{\psi_{CO_2}}{\psi_{CO}}\right)^2 + 4\psi_{CO}\left(\sqrt{K_6} + 2\frac{\psi_{CO_2}}{\psi_{CO}} K_1\right)\frac{\psi_{CO_2}}{\psi_{CO}} K_1}}{\psi_{CO}}$$

HP-65
PROGRAM

Input K_1 → STO 1	RCL 5	$f^{-1}\sqrt{\ }$	RCL 7
Input K_4 → STO 2	x	STO 8	RCL 8
Input K_6 → STO 3	RCL 6	RTN → p	$f\sqrt{\ }$
Input a → STO 4 Input ψ_{CO}	x	LBL B	÷
LBL A	1	RCL 4	R/S → ψ_{NO}
STO 5	RCL 4	RCL 5	2
RCL 4 RCL 5	−	−	÷
÷	RCL 2	R/S → ψ_{CO_2}	CHS
1	x	RCL 6	1
−	$f\sqrt{\ }$	$f^{-1}\sqrt{\ }$	+
RCL 1	RCL 6	RCL 8	RCL 4
x	x	÷	−
STO 6	STO 7	R/S → ψ_{O_2}	RTN → ψ_{N_2}
2	$f^{-1}\sqrt{\ }$	RCL 6	
x	÷	RCL 8	
RCL 3	$f\sqrt{\ }$	÷	
$f\sqrt{\ }$	RCL 7	RCL 3	
+	+	$f\sqrt{\ }$	
4	RCL 5	x	
x	÷	R/S → ψ_O	

3000°K					2700°K		
a = 0.1:	p:	100	30	10	100	30	10
	ψ_{CO}:	0.03864	0.05023	0.06168	0.02000	0.02762	0.03640
	ψ_{CO_2}:	0.06136	0.04977	0.03832	0.08000	0.07238	0.06360
	ψ_{O_2}:	0.00294	0.00382	0.00450	0.00164	0.00235	0.00313
	ψ_{O}:	0.00065	0.00136	0.00255	0.00016	0.00034	0.00068
	ψ_{NO}:	0.00624	0.00712	0.00773	0.00312	0.00373	0.00431
	ψ_{N_2}:	0.89688	0.89644	0.89614	0.89844	0.89813	0.89784
a = 0.03		0.01593	0.01959	0.02277	0.00887	0.01182	0.01497
		0.01407	0.01041	0.00723	0.02113	0.01818	0.01503
		0.00091	0.00110	0.00117	0.00058	0.00081	0.00103
		0.00036	0.00073	0.00130	0.00009	0.00020	0.00039
		0.00360	0.00396	0.00409	0.00193	0.00227	0.00257
		0.96820	0.96802	0.96795	0.96904	0.96886	0.96872
a = 0.01		0.00678	0.00787	0.00867	0.00415	0.00529	0.00639
		0.00322	0.00213	0.00133	0.00585	0.00471	0.00361
		0.00026	0.00029	0.00027	0.00020	0.00027	0.00033
		0.00019	0.00037	0.00062	0.00005	0.00012	0.00022
		0.00195	0.00204	0.00199	0.00115	0.00133	0.00146
		0.98902	0.98898	0.98900	0.98942	0.98934	0.98927

		2400°K			2100°K		
a = 0.1:	p:	100	30	10	100	30	10
	ψ_{CO}:	0.00783	0.01121	0.01548	0.00217	0.00316	0.00444
	ψ_{CO_2}:	0.09217	0.08879	0.08452	0.09783	0.09684	0.09556
	ψ_{O_2}:	0.00067	0.00101	0.00144	0.00019	0.00029	0.00042
	ψ_2:	0.00002	0.00005	0.00011	0.00000	0.00000	0.00001
	ψ_{NO}:	0.00120	0.00148	0.00177	0.00033	0.00041	0.00050
	ψ_{N_2}:	0.89940	0.89926	0.89912	0.89983	0.89979	0.89975
a = 0.03:		0.00363	0.00510	0.00688	0.00103	0.00147	0.00205
		0.02637	0.02490	0.02312	0.02897	0.02853	0.02795
		0.00025	0.00038	0.00054	0.00007	0.00011	0.00017
		0.00001	0.00003	0.00007	0.00000	0.00000	0.00001
		0.00077	0.00095	0.00113	0.00022	0.00027	0.00033
		0.96961	0.96953	0.96944	0.96989	0.96986	0.96983
a = 0.01:		0.00180	0.00247	0.00324	0.00052	0.00074	0.00102
		0.00820	0.00753	0.00676	0.00948	0.00926	0.00898
		0.00010	0.00015	0.00021	0.00003	0.00005	0.00007
		0.00001	0.00002	0.00004	0.00000	0.00000	0.00000
		0.00049	0.00060	0.00070	0.00014	0.00018	0.00022
		0.98976	0.98970	0.98965	0.98993	0.98991	0.98989

1800^oK

a		p: 100	30	10
a = 0.1:	ψ_{CO}:	0.00037	0.00055	0.00078
	ψ_{CO_2}:	0.09963	0.09945	0.09922
	ψ_{O_2}:	0.00003	0.00005	0.00008
	ψ_{O}:	0.00000	0.00000	0.00000
	ψ_{NO}:	0.00006	0.00007	0.00009
	ψ_{N_2}:	0.89997	0.89996	0.89996
a = 0.03:		0.00018	0.00026	0.00036
		0.02982	0.02974	0.02964
		0.00001	0.00002	0.00003
		0.00000	0.00000	0.00000
		0.00004	0.00005	0.00006
		0.96998	0.96998	0.96997
a = 0.01		0.00009	0.00013	0.00018
		0.00991	0.00987	0.00982
		0.00001	0.00001	0.00001
		0.00000	0.00000	0.00000
		0.00002	0.00003	0.00004
		0.98999	0.98998	0.98998

For a = 0: $$2\psi_{H_2O} + 2\psi_{H_2} + \frac{K_3\psi_{H_2O}}{\sqrt{p\psi_{H_2}}} + \sqrt{\frac{K_5\psi_{H_2}}{p}} = b$$

$$\psi_{H_2O} + \sqrt{\frac{K_4}{p}\left(1 - \frac{b}{2}\right)}K_2 \frac{\psi_{H_2O}}{\psi_{H_2}} + \frac{2}{p}\left(K_2 \frac{\psi_{H_2O}}{\psi_{H_2}}\right)^2 + \frac{\sqrt{K_6}}{p} K_2 \frac{\psi_{H_2O}}{\psi_{H_2}} + \frac{K_3\psi_{H_2O}}{\sqrt{p\psi_{H_2}}} = \frac{b}{2}$$

$$\therefore \quad \frac{2\psi_{H_2}^2}{b}\left[1 + \frac{1}{2}\sqrt{\frac{K_5}{p\psi_{H_2}}}\right] =$$

$$\frac{\left(\sqrt{\frac{K_4}{p}\left(1 - \frac{b}{2}\right)} + \frac{\sqrt{K_6}}{p}\right)K_2 + \dfrac{\frac{b}{p} K_2^2}{\psi_{H_2} + \frac{K_3}{2}\sqrt{\psi_{H_2}/p}} + \frac{K_3}{2}\sqrt{\frac{\psi_{H_2}}{p}}}{1 + \frac{1}{\psi_{H_2}}\left[\left(\sqrt{\frac{K_4}{p}\left(1 - \frac{b}{2}\right)} + \frac{\sqrt{K_6}}{p}\right)K_2 + \frac{K_2^2}{p}\dfrac{2b - 2\psi_{H_2} - \sqrt{\frac{K_5\psi_{H_2}}{p}}}{\psi_{H_2} + \frac{K_3}{2}\sqrt{\frac{\psi_{H_2}}{p}}} + K_3\sqrt{\frac{\psi_{H_2}}{p}}\right]}$$

Input K_2 → STO 1	RCL 5	STO 8	x	RCL 4	RCL 1
Input K_3 → STO 2	f$\sqrt{\ }$	RTN	2	RCL 7	x
Input K_4 → STO 3	RCL 7	LBL B RCL 9	x	÷	RCL 7 ÷
Input K_5 → STO 4	÷	RCL 8 RCL 8	RCL 7	RCL 8	RCL 8
Input K_6 → STO 5	+	RCL 7	÷	÷	gx >< y
Input $\frac{b}{2}$ → STO 6	RCL 1	÷	RCL 1	f$\sqrt{\ }$	STO 8
Input p → STO 7	x	f$\sqrt{\ }$	x	2	gRv
LBL A	STO 9	2	RCL 1	÷	RCL 7
1	RCL 1	÷	x	1	x
RCL 6	$f^{-1}\sqrt{\ }$	RCL 2	+	+	f$\sqrt{\ }$
−	RCL 7	x	+	÷	g 1/x
RCL 3	÷	+	gLSTx	gx >< y	2
x	+	gLSTx	2	gLSTx	÷
RCL 7	RCL 6	gx >< y	x	2	RCL 2
÷	x	g 1/x	RCL 9	x	x
f$\sqrt{\ }$	f$\sqrt{\ }$	RCL 6	+	RCL 1	1
			RCL 8	x	+
			÷ gx >< y	RCL 8 ÷	

RCL 8	LBL C	+	+	RCL 9	RCL 7
gx >< y	RCL 8	RCL 8	÷	RCL 1	÷
÷	RCL 4	–	R/S → ψ_{H_2O}	x	f$\sqrt{\ }$
–	x	RCL 2	STO 9	RCL 8	R/S → ψ_O
1	RCL 7	2	RCL 2	÷	1
+	÷	÷	x	$f^{-1}\sqrt{\ }$	RCL 6
÷	f$\sqrt{\ }$	RCL 8	RCL 8	RCL 7	–
RCL 6	R/S → ψ_O	RCL 7	RCL 7	÷	RCL 9
x	CHS	x	x	R/S → ψ_{O_2}	x
f$\sqrt{\ }$	2	f$\sqrt{\ }$	f$\sqrt{\ }$	STO 9	RCL 3
STO 8	÷	÷	÷	RCL 5	x
RTN	RCL 6	1	R/S → ψ_{OH}	x	f$\sqrt{\ }$
					R/S → ψ_{NO}
					CHS
					2
					÷
					RCL 6
					–
					1
					+
					RTN → ψ_{N_2}

	p:	3000°K 100	30	10	2700°K 100	30	10
b/2 = 0.03	ψ_{H_2}:	0.00463	0.00610	.00762	.00231	.00318	.00421
	ψ_H:	0.00107	0.00224	.00434	.00027	.00058	.00116
	ψ_{H_2O}:	0.02399	0.02156	.01858	.02718	.02598	.02443
	ψ_{OH}:	0.00171	0.00244	.00326	.00074	.00110	.00156
	ψ_{O_2}:	0.00058	.00089	.00127	.00031	.00049	.00075
	ψ_O:	0.00029	.00065	.00135	.00007	.00016	.00033
	ψ_{NO}:	0.00287	.00357	.00426	.00140	.00178	.00219
	ψ_{N_2}:	0.96857	.96822	.96787	.96930	.96911	.96891
b/2 = 0.01		.00228	.00284	.00331	.00119	.00159	.00204
		.00075	.00153	.00286	.00020	.00041	.00081
		.00699	.00590	.00464	.00855	.00796	.00723
		.00071	.00098	.00124	.00032	.00048	.00066
		.00020	.00031	.00042	.00011	.00018	.00028
		.00017	.00038	.00078	.00004	.00010	.00020
		.00172	.00212	.00248	.00086	.00110	.00135
		.98914	.98894	.98876	.98957	.98945	.98933
b/2 = 0.003		.00100	.00114	.00116	.00057	.00073	.00088
		.00050	.00097	.00170	.00014	.00028	.00053
		.00163	.00122	.00081	.00230	.00204	.00173
		.00025	.00032	.00036	.00013	.00018	.00024
		.00006	.00008	.00010	.00004	.00006	.00008
		.00009	.00020	.00039	.00002	.00005	.00011
		.00092	.00110	.00123	.00048	.00061	.00075
		.99654	.99645	.99638	.99676	.99669	.99663

2400°K				2100°K		
b/2 = 0.03	100	30	10	100	30	10
	.00092	.00129	.00176	0.00027	.00039	.00053
	.00005	.00010	.00021	.00001	.00001	.00002
	.02893	.02847	.02786	.02970	.02956	.02939
	.00025	.00037	.00054	.00006	.00009	.00013
	.00013	.00021	.00033	.00004	.00007	.00011
	.00001	.00002	.00005	.00000	.00000	.00000
	.00055	.00071	.00088	.00016	.00021	.00026
	.96972	.96965	.96956	.96992	.96990	.96987
b/2 = 0.01	.00049	.00067	.00090	.00015	.00020	.00028
	.00004	.00008	.00015	0.00000	.00001	.00002
	.00944	.00921	.00891	.00984	.00977	.00968
	.00011	.00017	.00024	.00003	.00004	.00006
	.00005	.00008	.00013	.00002	.00003	.00004
	.00001	.00002	.00003	.00000	.00000	.00000
	.00034	.00044	.00056	.00010	.00013	.00017
	.98983	.98978	.98972	.98995	.98993	.98992
b/2 = 0.003	.00024	.00033	.00043	.00005	.00010	.00014
	.00002	.00005	.00010	.00000	.00001	.00001
	.00272	.00261	.00247	.00292	.00289	.00284
	.00004	.00007	.00010	.00001	.00002	.00002
	.00002	.00003	.00004	.00001	.00001	.00002
	.00000	.00001	.00002	.00000	.00000	.00000
	.00020	.00026	.00032	.00006	.00008	.00010
	.99690	.99687	.99684	.99697	.99696	.99695

1800°K

b/2 = 0.03	100	30	10
	.00005	.00008	.00011
	.00000	.00000	.00000
	.02994	.02992	.02988
	.00001	.00001	.00002
	.00001	.00001	.00002
	.00000	.00000	.00000
	.00003	.00004	.00005
	.96998	.96998	.96997

b/2 = 0.01	.00003	.00004	.00006
	.00000	.00000	.00000
	.00997	.00996	.00994
	.00000	.00001	.00001
	.00000	.00001	.00001
	.00000	.00000	.00000
	.00002	.00003	.00003
	.98999	.98999	.98998

General case: $(a,b \neq 0)$

$$\psi_{H_2O} = \frac{\frac{b}{2} - \psi_{H_2} - \frac{1}{2}\sqrt{\frac{K_5\psi_{H_2}}{p}}}{1 + \frac{K_3}{2\sqrt{p\psi_{H_2}}}}, \quad \psi_{CO} = \frac{a}{1 + \frac{K_2}{K_1}\frac{\psi_{H_2O}}{\psi_{H_2}}}$$

$$\psi_{H_2} = \frac{\psi_{H_2O}}{p} \; \frac{K_2\left[\sqrt{pK_4(1-a-\frac{b}{2})} + \sqrt{K_6}\right] + \frac{K_3}{2}\sqrt{p\psi_{H_2}} + \frac{2K_2^2\psi_{H_2O}}{\psi_{H_2}}}{\psi_{CO} + \psi_{H_2} + \frac{1}{2}\sqrt{\frac{K_5\psi_{H_2}}{p}}}$$

$b/2 \rightarrow$ STO 1	CHS	$f^{-1}\sqrt{\ }$	−	÷	+
$p \rightarrow$ STO 2	RCL1	x	R/S	RCL3	÷
$K_1 \rightarrow$ STO 4	+	RCL3	a	÷	RTN$\rightarrow\psi_{H_2}$
$K_2 \rightarrow$ STO 5	RCL6	÷	R/S	1	a
$K_3 \rightarrow$ STO 6	2	RCL3	−	+	B
$K_4 \rightarrow$ STO 7	÷	RCL2	RCL7	R/S	LBL B
$K_5 \rightarrow$ STO 8	RCL2	x	x	a	RCL3
$K_6 \rightarrow$ STO 9	RCL3	$f\sqrt{\ }$	RCL2	R/S	R/S$\rightarrow\psi_{H_2O}$
ψ_{H_2}	x	RCL6	x	$gx \gtrless y$	RCL3
A	$f\sqrt{\ }$	x	$f\sqrt{\ }$	÷	gR∧
STO 3	÷	2	RCL5	RCL3	STO 3
RCL8	1	÷	x	+	÷
x	+	+	+	RCL3	RCL5
RCL2	÷	RCL9	RCL2	gR∧	x
÷	2	$f\sqrt{\ }$	÷	STO 3$\rightarrow\psi_{H_2O}$	RCL4
$f\sqrt{\ }$	$gx \gtrless y$	RCL5	x	gRV	÷
2	x	x	$gx \gtrless y$	RCL8	1
÷	gLSTx	+	RCL5	RCL2	+
RCL3	$gx \gtrless y$	1	x	÷	gR∧
+	RCL5	RCL1	RCL4	$f\sqrt{\ }$	$gx \gtrless y$
				2	
				÷	

÷	g$x \gtrless y$	f$\sqrt{\ }$	f$^{-1}\sqrt{\ }$	RCL1	f$\sqrt{\ }$	R/S→ψ_O
R/S→ψ_{CO}	x	÷	RCL2	−	R/S→ψ_{NO}	RCL3
gR∧	gLSTx	R/S→ψ_{OH}	÷	g$x \gtrless y$	gR∨	RCL8
−	g$x \gtrless y$	gR∨	R/S→ψ_{O_2}	x	RCL9	x
CHS	RCL2	RCL3	g$x \gtrless y$	gLSTx	x	RCL2
R/S→ψ_{CO_2}	f$\sqrt{\ }$	÷	CHS	g$x \gtrless y$	RCL2	÷
gR∨	÷	RCL5	1	RCL7	÷	f$\sqrt{\ }$
RCL6	RCL3	x	+	x	f$\sqrt{\ }$	RTN→ψ_H

$a = 0.1$, $b/2 = 0.01$

	3000°K			2700°K		
p:	100	30	10	100	30	10
ψ_{H_2} :	.00044	.00061	.00081	.00022	.00031	.00043
ψ_{H_2O}:	.00842	.00766	.00668	.00933	.00899	.00853
ψ_{CO} :	.02767	.03713	.04738	.01361	.01905	.02567
ψ_{CO_2}:	.07233	.06287	.05262	.08639	.08095	.07433
ψ_{OH} :	.00195	.00274	.00358	.00083	.00122	.00170
ψ_{O_2} :	.00798	.01116	.01441	.00413	.00617	.00861
ψ_{NO} :	.01023	.01209	.01374	.00492	.00602	.00711
ψ_O :	.00107	.00232	.00456	.00025	.00055	.00113
ψ_H :	.00033	.00071	.00142	.00008	.00018	.00037

2400°K | 2000°K

p:	100	30	10	100	30	10
ψ_{H_2}:	.00009	.00013	.00018	.00002	.00004	.00006
ψ_{H_2O}:	.00977	.00966	.00950	.00994	.00991	.00987
ψ_{CO}:	.00517	.00746	.01032	.00141	.00205	.00288
ψ_{CO_2}:	.09483	.09254	.08968	.09859	.09795	.09712
ψ_{OH}:	.00027	.00040	.00057	.00006	.00009	.00013
ψ_{O_2}:	.00162	.00247	.00364	.00045	.00070	.00104
ψ_{NO}:	.00186	.00230	.00279	.00051	.00064	.00078
ψ_{O}:	.00004	.00008	.00018	.00000	.00001	.00002
ψ_{H}:	.00001	.00003	.00007	.00000	.00000	.00001

1800°K

.00001	.00001	.00001
.009990	.009985	.009978
.00024	.00036	.00050
.09976	.09965	.09950
.00001	.00001	.00002
.00008	.00012	.00018
.00009	.00011	.00014
.00000	.00000	.00000
.00000	.00000	.00000

Gasdynamics: $M\varepsilon = \left[\frac{2}{\gamma+1}\left(1 + \frac{\gamma-1}{2}M^2\right)\right]^{\frac{\gamma+1}{2(\gamma-1)}}$, $\frac{T^*}{T} = \frac{2}{\gamma+1}\left(1 + \frac{\gamma-1}{2}M^2\right)$

$$\left[(M\varepsilon)^{\frac{2(\gamma-1)}{\gamma+1}} \frac{\gamma+1}{2} - 1\right]\frac{2}{\gamma-1} = M^2 \rightarrow \frac{T^*}{T} = (M\varepsilon)^{\frac{2(\gamma-1)}{\gamma+1}}$$

$\gamma \rightarrow$ STO 1	RCL 1	RCL 2	÷	$f\sqrt{\ }^{-1} \rightarrow M$	gy^x
ε	1	x	1	STO 4	RTN $\rightarrow \frac{T^*}{T}$
LBL A	+	RCL 3	–	R/S	
STO 2	÷	gy^x	2	GTO 1	
RCL 1	STO 3 $\rightarrow \frac{2(\gamma-1)}{\gamma+1}$	RCL 1	x	LBL B	
1	1	1	RCL 1	RCL 4	
–	STO 4	+	1	RCL 2	
2	LBL 1	x	–	x	
x	RCL 4	2	÷	RCL 3	

$\gamma = 1.4$

ε:	10	20	30	40	50	60
M:	3.9225	4.7255	5.2310	5.6087	5.9138	6.1713
T*/T:	3.3977	4.5550	5.3939	6.0763	6.6621	7.1809

$\gamma = 1.3$

M:	3.5824	4.2070	4.5863	4.8635	5.0838	5.2674
T*/T:	2.5435	3.1782	3.6132	3.9549	4.2407	4.4886

APPENDIX C

THEME TEAM 7

MULTIPURPOSE SPACE POWER PLATFORM

Prepared by:

Robert E. Ricles

John D. G. Rather

Derek Teare

Herb Williams

THEME TEAM 7

MULTIPURPOSE SPACE POWER PLATFORM

Report of
Presentation to Theme Team Members
at
NASA/OAST

on
11 May 1976

Prepared for:

NASA/OAST
NASA Contract NASW-2866
Modification No. 1

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CONTENTS

Introduction and Summary

NASA/OAST has recently been studying a number of exciting potential future space opportunities which could be technology driven and thus could be a major influence to the Research and Technology program. These opportunities were organized into "Themes" and Theme 07 - Multipurpose Space Power Platform (MSPP) anticipated the use of High Power Lasers as one of the possible power sources.

Based on its ongoing work under NASA Contract NASW-2866, W. J. Schafer Associates, Inc. (WJSA) was given the following additional tasks to support Theme Team 7's efforts:

1. Derive estimates of the requirements for laser-powered propulsion for orbit-raising;
2. Derive estimates of the requirements for laser-power transmission; and
3. Derive rough cost estimates.

The results were reported at a Theme Team Meeting at NASA/OAST on 11 May 1976. This annotated viewgraph report documents that presentation and is in conformance with the requirements of Modification No. 1 of NASA Contract NASW-2866.

Figure 1. NASA identified the three MSPP applications shown. The laser is probably not an important contender for space manufacturing since its major advantages over welding methods such as e-beam, for example, in terrestial applications is that it does not require a vacuum. Our efforts were concentrated primarily on the third question — What are the Laser System Requirements?

- WHAT ARE THE MSPP APPLICATIONS?
  - POWER TRANSMISSION TO SPACE VEHICLES AND SPACE STATIONS
  - POWER TRANSMISSION FOR PROPULSION
  - MANUFACTURING
- IN WHICH OF THESE MIGHT LASER SYSTEMS PROVE USEFUL?
  - POWER TRANSMISSION
  - LASER PROPULSION
- WHAT ARE THE LASER SYSTEM REQUIREMENTS?
- HOW DOES THE LASER COMPARE WITH OTHER METHODS?
- WHAT ADVANCES IN THE STATE-OF-THE-ART ARE REQUIRED?

FIGURE 1 — EVALUATE THE ROLE OF LASER SYSTEMS IN THE MULTIPURPOSE SPACE POWER PLATFORM (MSPP)

Figure 2. On this chart are enumerated the specific applications under each of the major broad categories. Although most of these have been mentioned by NASA or others in one source or another as potential uses, we believe a couple are unique to this presentation. For example, if there were a scientific or other need for a satellite that spent the majority of its time in darkness, MSPP could provide the power. Similarly, for either peak loads or eclipse, where power storage is currently required, MSPP could replace the power storage system. More detailed tradeoffs are required to test the efficiency of these applications and to derive the cost benefits.

Also, in the event of an attack by enemy forces, the laser system could probably be used to defend our valuable property; whereas, other systems, such as microwave, could not.

- PROPULSION
  - ORBIT CHANGING
  - ATTITUDE CONTROL AND STATION KEEPING
  - LUNAR PAYLOADS
  - ESCAPE PAYLOADS
  - AIRCRAFT FUELING FOR LOITER MISSIONS
  - LIGHTER-THAN-AIR CRAFT
- POWER TRANSMISSION
  - REPLACE POWER SUPPLY (SOLAR PANELS, ETC.)
  - REPLACE POWER STORAGE (ILLUMINATE SOLAR PANELS WHEN ECLIPSED)
  - REPLACE POWER STORAGE FOR PEAK LOADS
  - MAKES AVAILABLE OPPORTUNITY FOR SPECIAL ORBITS WHERE SATELLITE SPENDS MOST OF TIME IN DARKNESS
  - POWER MAJOR STATIONS (REMOTE NUCLEAR STATION)
- DEFENSE IMPLICATIONS
  - COULD DEFEND VALUABLE PROPERTY FROM UNWANTED INTRUDERS

FIGURE 2 — CANDIDATE APPLICATIONS FOR LASERS FROM MSPP

LASER PROPULSION

Figure 3. For this chart we have derived the power requirements for three different cases: 1) the payload is delivered from low earth orbit (LEO) to geosynchronous orbit (GEO) and the vehicle is expended or remains at GEO; 2) the payload is delivered from LEO to GEO and returns back to LEO empty; and 3) the payload is delivered from LEO to GEO, an exchange of payload is made, and then the vehicle with the new payload is returned from GEO to LEO.

In each case it is assumed that the initial mass in LEO is 2.7×10^4 kg, and that the vehicle dry mass is 3700 kg plus tankage, taken as 5% of the fuel mass. (These values are similar to those used in Ref. 1.) The required velocity increment is taken to be 5631 m/sec for the transfer in each direction. This value is sufficient for transfer from an inclined low orbit to an equatorial GEO when all velocity changes are made impulsively; it is not strictly valid when the orbit changes are achieved by continuous application of low thrust levels, but is a reasonable assumption for the present purpose. Finally, an overall nozzle coefficient $C_N = 0.64$ is assumed (as in Ref. 2), so that the energy delivered by the laser is $(G_0 I_{SP})^2/2C_N$ j/kg of propellant consumed.

For each case it is then possible to determine payload as a function of specific impulse, and an optimum value of I_{SP} can be established which minimizes the required laser energy per kilogram of payload. Typical examples are plotted for the above cases; along each line the total laser energy remains constant, so that power required is inversely proportional to irradiation time.

In case 1) (the one-way trip) a specific impulse of 712 seconds is best with an expenditure of 75 megajoule per kilogram of payload. For the second case, LEO to GEO and return empty, the optimum is at a specific of 1300 seconds and 140 megajoules per kilogram. And finally, for the last case a

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Ref. 1 - M. A. Minovitch, "Performance Analysis of a Laser-Propelled Interorbital Transfer Vehicle," NASA CR-134966, Feb. 1976.

Ref. 2 - F. E. Rom & H. A. Putre, "Laser Propulsion," Lewis Research Center TMX-2510, June 1972.

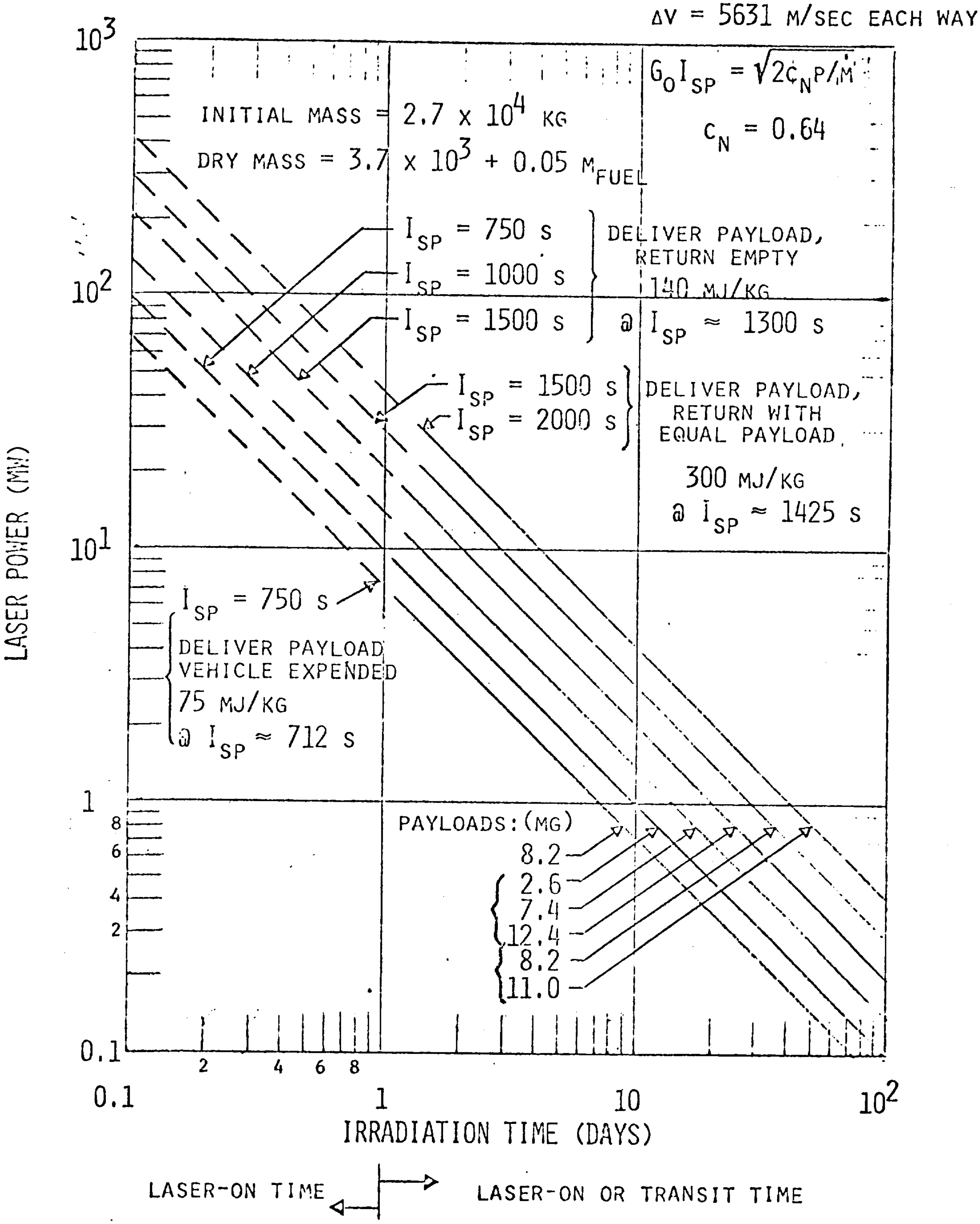

FIGURE 3 — POWER REQUIREMENT FOR PAYLOAD DELIVERY FROM LEO TO GEO

specific of 1425 is required and 300 megajoules per kilogram. For irradiation times greater than somewhere around one day, we can assume that the vehicle is constantly thrusting and constantly being irradiated. For times less than about a day, due to eclipse, need for coasting, etc., we can only assume that the time is a laser-on time. The important message from this viewgraph is that if we consider that a transit time on the order of 1 day is desirable, then powers on the order of 10-50 megawatts are required, which certainly are not unreasonable.

Figure 4. In assessing the value of laser propulsion, one has to know that the competition is capable of and the solar tug has to be considered competitive. If one assumes a 10 megawatt collector, then the diameter of the collector is 97 meters; although this at first blush appears rather large, it should be noted that it can be a fairly low quality collector. This size collector would then do the same job as the laser propulsion in the previous chart and the specific impulse would be around 1000 seconds.

10 MW COLLECTOR, D = 97M

HYDROGEN AT 3000°K, ΔH = 48.6 MJ/KG

FLOW RATE = 10 MJ/S $\div$ 48.6 MJ/KG = 0.206 KG/S

SPECIFIC IMPULSE $\sim$ 1000 SEC

FIGURE 4 — SOLAR TUG

Figure 5. In this figure we show the transmitter diameter versus the receiver diameter assuming that all the energy within the $1 - 1/e^2$ points is collected. Three different wavelengths are shown. Also plotted on the abscissa is the maximum allowable beam spread due to jitter (σ_J) to collect the energy. We have assumed that (σ_D) is equal to the beam spread due to diffraction (σ_D) and the beam quality is twice diffraction limited. For most purposes, to get the total beam spread, it is acceptable to root sum square the jitter and diffraction errors as we have done in some later plots. It is interesting to determine a weight optimization for the combined receiver and transmitter mirrors. If one assumes a scaling law where the weight is proportional to the diameter to some power (in this case we use the 2.5 power) then, for a single receiver and a single transmitter, we have minimum weight when these two diameters are equal. On the other hand for a case where we have 300 receivers and one transmitter, the optimization is as shown, (i.e., $D_T \sim 10\ D_R$) providing the proportionality constant is the same for both types of mirror. The receiver mirror does not have to be as well figured as the transmitter mirror so that we can probably assume that a receiver would weigh less than a transmitter of the same diameter. Thus, the $D_T/D_R = 10$ line would be more appropriate to minimum total weight for a system with more than 300 receivers.

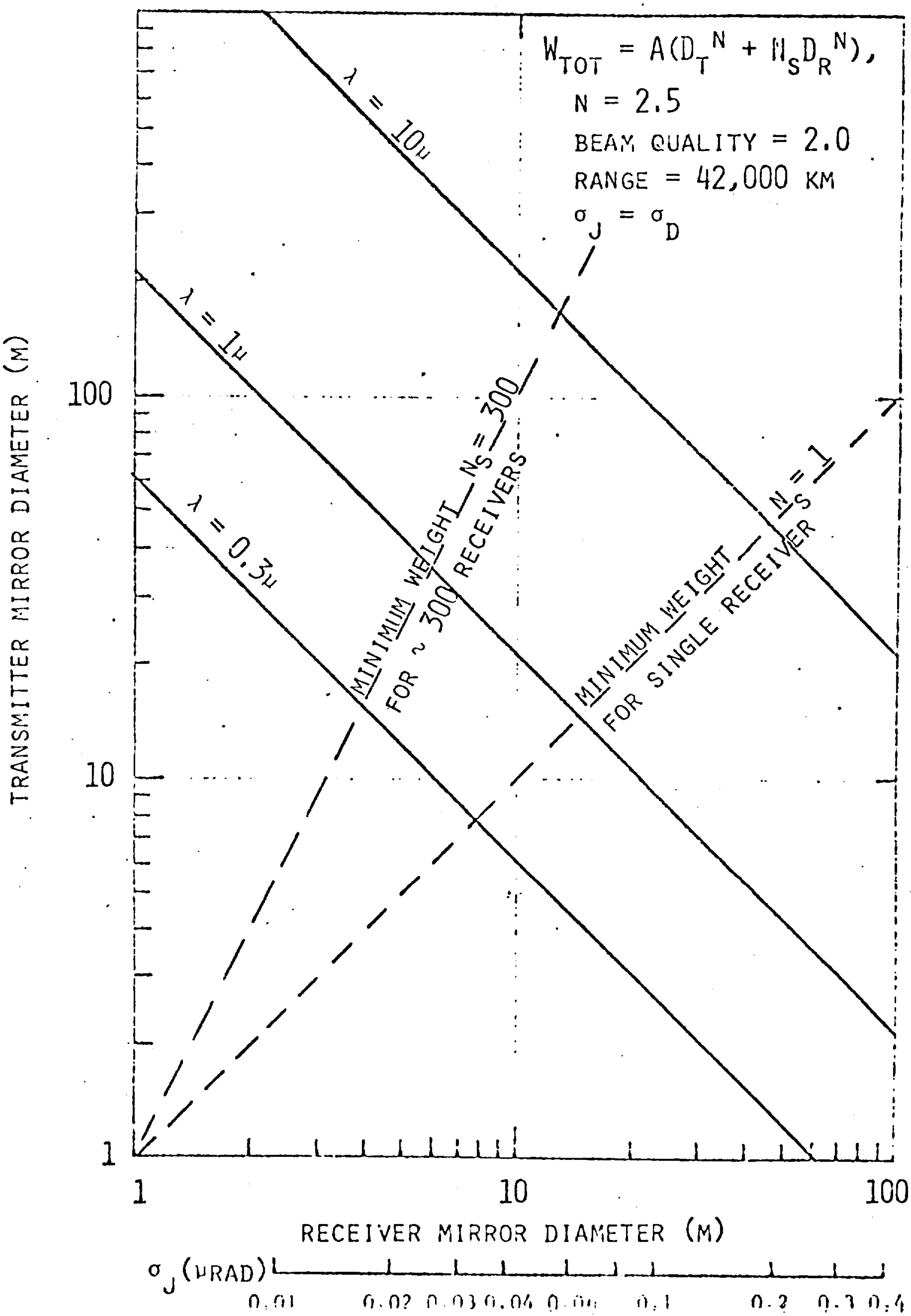

FIGURE 5 — MIRROR DIAMETERS FOR LASER POWER TRANSMISSION

Figure 6. Here we show the assumptions used in deriving the curves on the previous figure. The maximum range could be twice the range from LEO to GEO for the case where the satellite is on the other side of the earth.

ASSUMPTIONS: $\sigma_J \approx \sigma_D$

INTERCEPT GAUSSIAN INTENSITY AT TARGET PLANE TO $1/E^2$ POINTS

$D_T D_R \geq 8 \eta\lambda R/\pi$ η = BEAM QUALITY

LEO → GEO: $R = 42 \times 10^6 \pm 6 \times 10^6$ M

$\eta = 2$

$\lambda = 10$ μM

GIVES $D_T D_R \geq 2100$ M^2

$D_T \approx D_E \approx 46$ M

23M SUBTENDS ~ 0.5 μRAD AT 42×10^6 M RANGE

∴ POINTING ACCURACY → 0.1 μRAD

LASER LOCATED AT GEO: $R_{MAX} \rightarrow 84 \times 10^6$ M

FIGURE 6 — TRANSMISSION OF LASER POWER TO LASER PROPULSION ENGINE

LASER POWER

Figure 7. In Figure 7, we have derived a "carpet plot" for laser power in which we show all of the pertinent parameters required to size a system for providing power from a single MSPP to any number of other collectors. We will give two examples on the use of this chart. In the first one, let us assume that we require electrical power on board our receiving station of 1 megawatt. Let us further assume that we have a 10% conversion efficiency from received power to electrical power. On this basis, we would then enter the upper left-hand abscissa at the 10 megawatt point. If we move to the lower left hand abscissa, we would see that this corresponds to a transmitted power somewhere in the order of 11 1/2 megawatts. Moving down and saying that our transmitter aperture should radiate all of its heat to space passively thus not requiring any active cooling system on board, we would find that it would require a transmitter aperture of the order of 20 meters. If we then assume that we have a beam quality factor of 2, we would move up on the lower right-hand coordinate to 10. Now let us assume that we have a laser operating at 5 microns. We then come across to that line and then up to the right-hand abscissa to determine that the maximum allowable beam spread (root sum square of the diffraction plus jitter spread) is in the order of 0.3. Let us further assume that we want to transmit from geosynchronous orbit for a distance of approximately 40,000 kilometers, we would then move up to the GEO line and across to the upper right-hand coordinate to determine that the receiver aperture has to be approximately 50 meters.

It is interesting to again assess what the competition could do. If one had a solar collector of this size, one would receive power of slightly over 1 megawatt, roughly a factor of 10 smaller than is desired.

Another one of the many possible ways of using this chart is the following: Let us assume that we know that the minimum beam spread that we can achieve

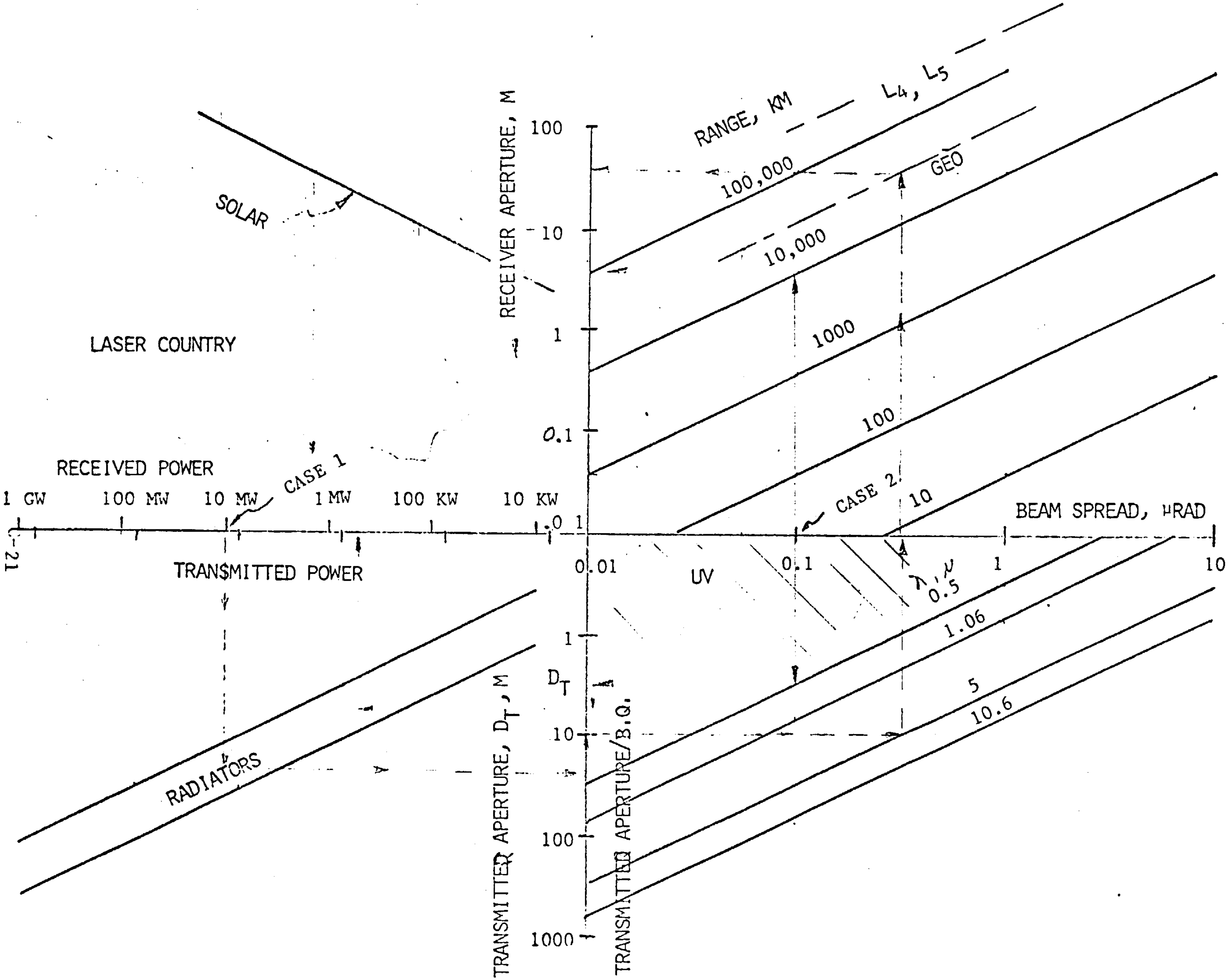

FIGURE 7 — LASER POWER "CARPET PLOT"

is a tenth of a micro-radian; we would then enter the chart at that point and let us further assume that we wanted to transmit over 10,000 kilometers and that we had a visible (.5 micron) laser. We would then find that we need a receiver aperture of about 4 meters and a transmitter aperture divided by beam quality of about 3 meters, if we multiply by a factor of 2 for the beam quality, we then find that we have a transmitter aperture of about 6 meters, and this in turn would give us in the order of 800 kilowatts of transmitted power to receive about 700 kilowatts of received power. The solar collector in this case would give us something/slightly more than 10 kilowatts of received power.

Also, the microwave could easily fit in this plot if the lower right-hand coordinate were increased by 4 orders of magnitude.

COST PROJECTIONS

Figure 8. Cost projections at this point in time for space-borne laser systems are at best nebulous. As a first approach to deriving some estimates we have plotted the cost versus the power for a number of existing and projected systems. The flagged symbols indicate closed-cycle systems whereas the others are open-cycle systems. These consist of various types of lasers including GDL's, chemical lasers and EDL's. None of these have been designed for or are being used in space applications. Most of the lasers shown are essentially one of a kind, and have the development costs included. Based on this plot one could infer that the cost of a laser system should be in the order of $\$10^4$/KW plus or minus an order of magnitude.

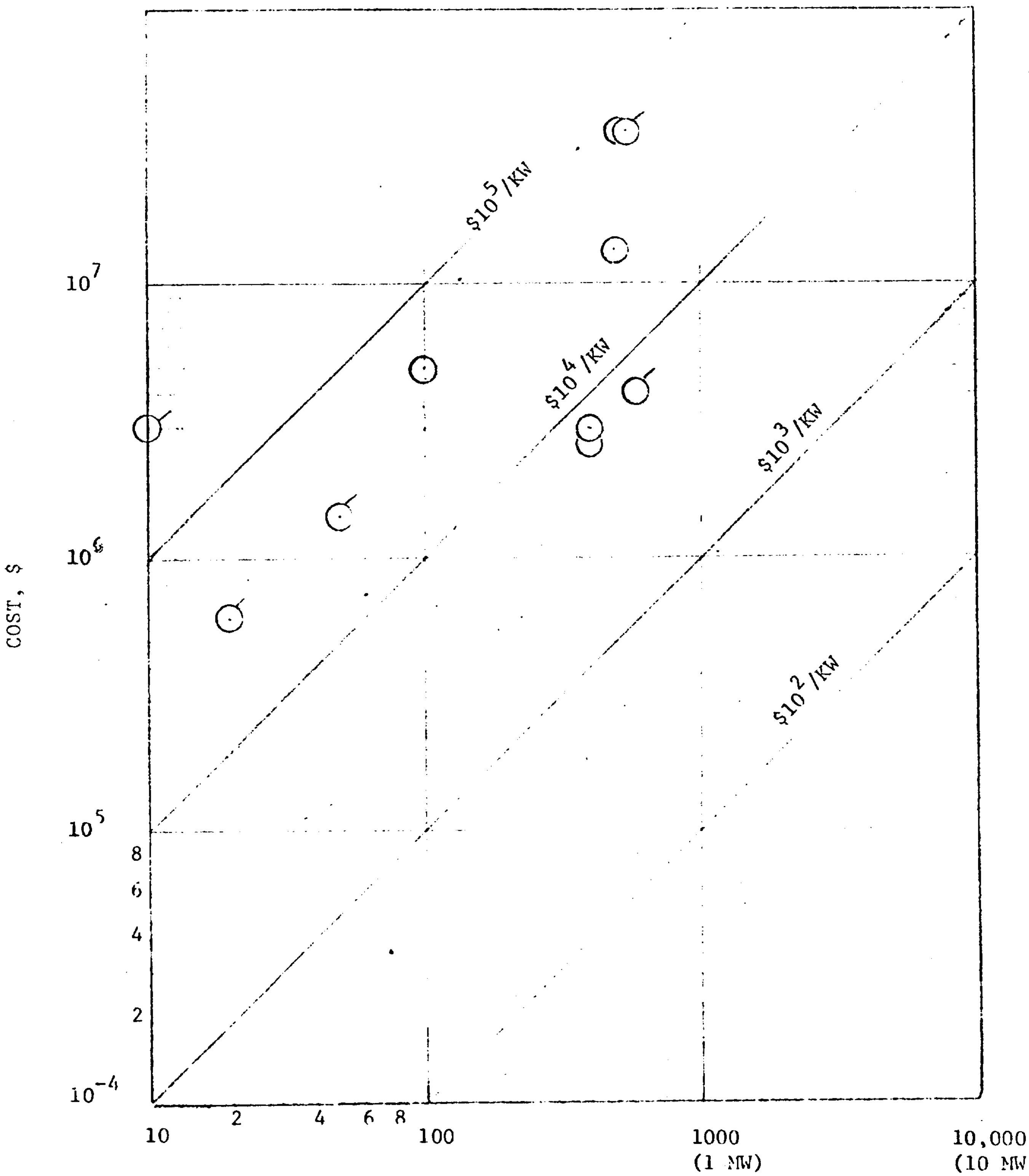

FIGURE 8 LASER COSTS

Figure 9. Based on the information contained in Figure 8, we can make further projections. If, to be conservative, we assume that the present cost would be up around $\$10^5$/KW, we can then project a decrease with the time based on the fact that development costs will have been written off against earlier systems and proof testing, etc., will have been completed so that, at some point in time--like ten years hence--the price should drop by an order of magnitude and should continue to drop as time goes on, based on improvements, etc.

An open cycle chemical laser ought to be about an order of magnitude more expensive based on the cost of the fuel and the fact that the fuel has to be transported up to the space station in order to recycle as it is required.

We have also shown the cost of solar collectors on this plot. This is based on projections given in the JPL reference manual 1975 and the solar cell cost has been increased by a factor of 5 in order to account for power conditioning, etc.

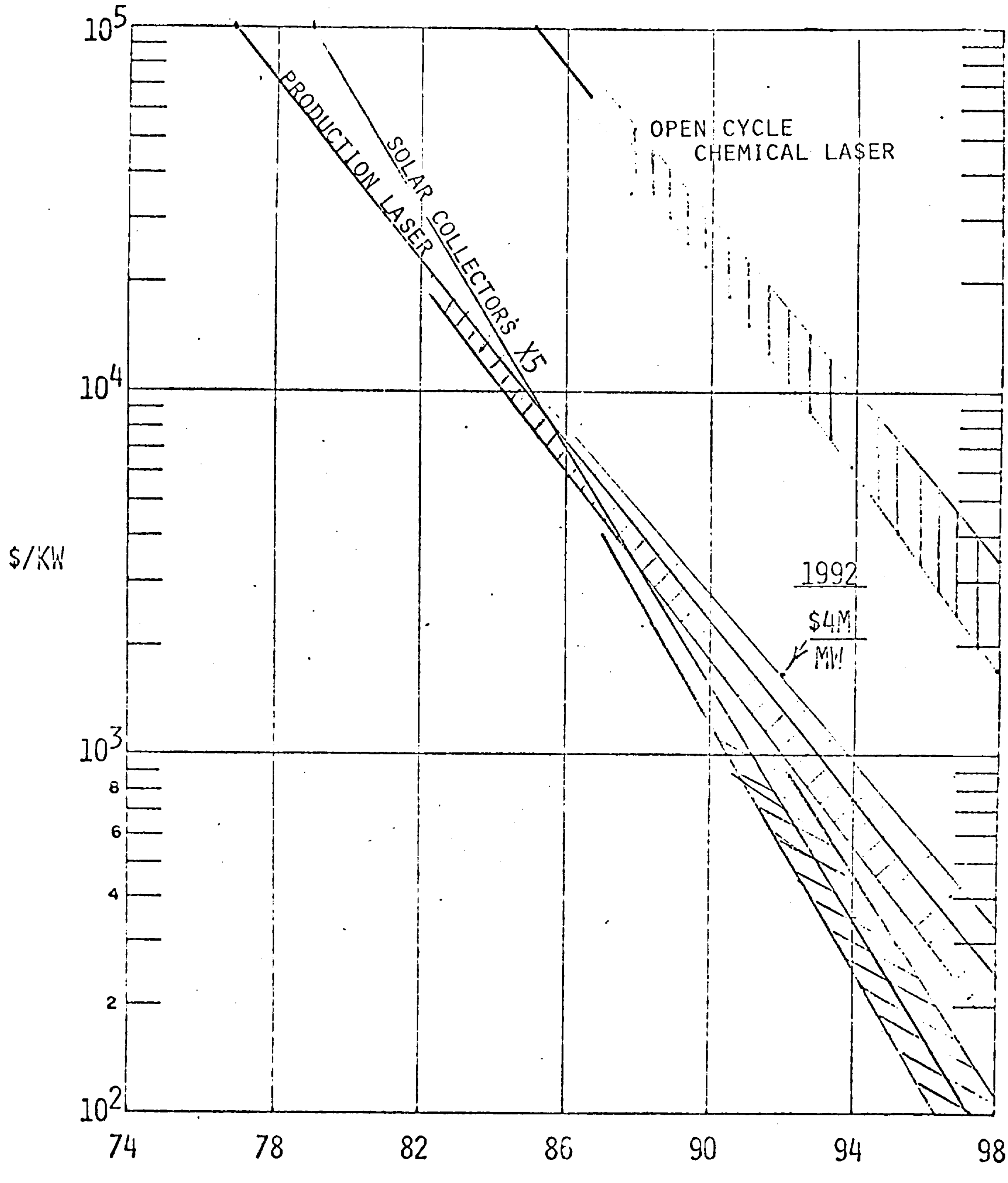

FIGURE 9 COST PROJECTIONS

Figure 10. We can now translate time into a projection of the power that might be available at those given times and thus provide cost projections in terms of \$/KW versus power in kilowatts. Again, to emphasize the point, there is a large error band on any of these cost projections.

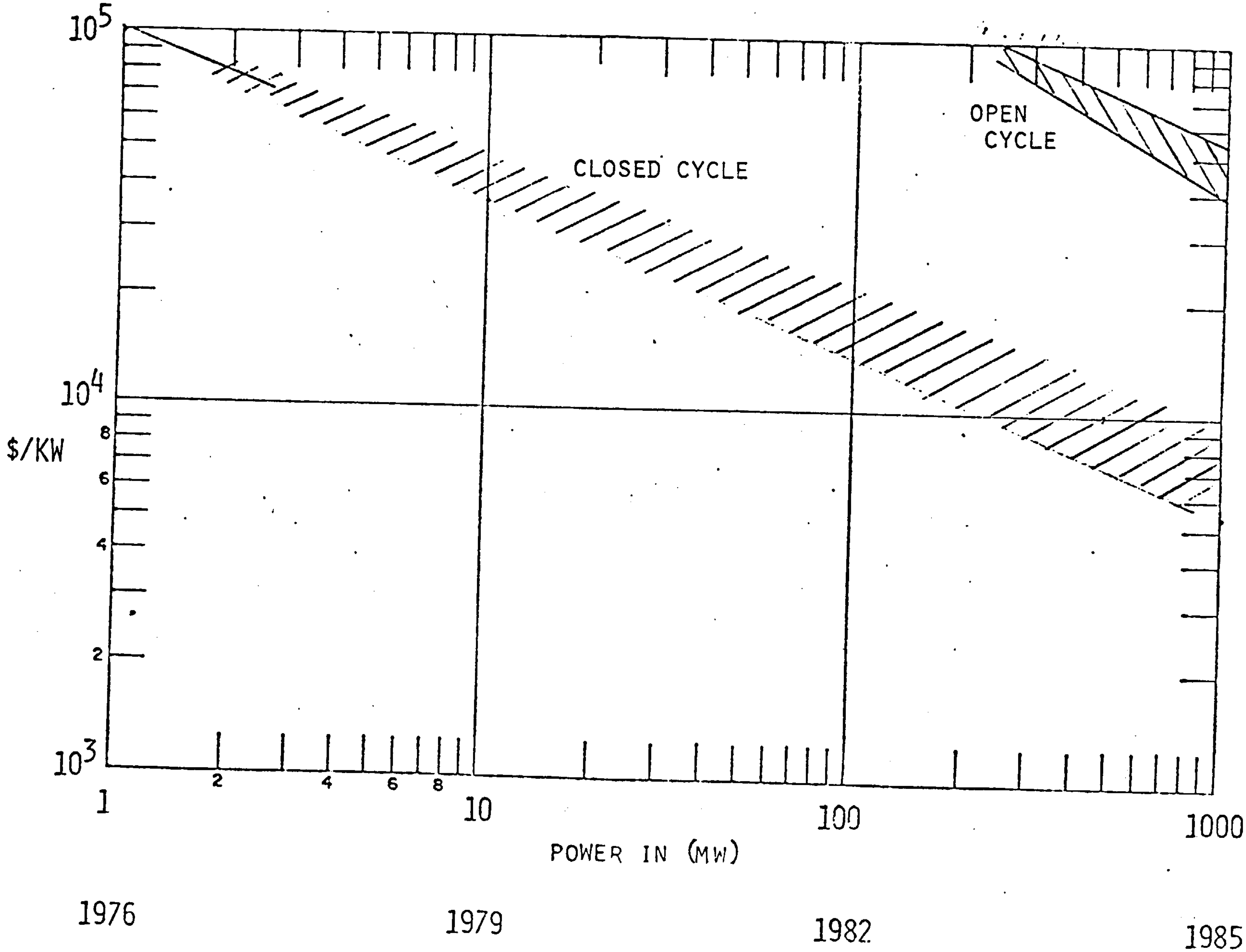

FIGURE 10 LASER COST PROJECTIONS

Figure 11. Finally, we can estimate rough order of magnitude funding requirements for the launch of a test-bed demonstration of roughly a 10 kilowatt laser system. The costs shown do not include the launch vehicle. Total cost over a five year span would be approximately $38 million dollars.

YEARS FROM GO AHEAD	1	2	3	4	5	
PLANNING AND CONCEPT DEFINITION STUDIES	$\$2 \times 10^6$					
DESIGN, FAB, DEMONSTRATE CRITICAL COMPONENTS		$\$14 \times 10^6$				
DESIGN, FAB, FAT FLIGHT HARDWARE			$\$14 \times 10^6$			
COMPATIBILITY AND INTERFACE TESTS LAUNCH PREPS AND LAUNCH				$\$4 \times 10^6$		
DATA ANALYSIS AND SUPPORT					$\$4 \times 10^6$	
						$\$38 \times 10^6$

FIGURE 11 ROM FUNDING REQUIREMENTS

APPENDIX D

TRIPS AND CONFERENCES

APPENDIX D

TRIPS AND CONFERENCES

W. J. Schafer Associates personnel participated in the following meetings and conferences during the conduct of the study.

DATE	FACILITY	PURPOSE OF VISIT AND PERSONNEL CONTACTED
11 November 1975	NASA/Ames Research Center Mountainview, CA	Technical Discussions - Dr. K. Billman
2,3 December 1975	National Science Foundation Auditorium, Washington, D.C.	Attend NASA Briefing - R. E. Smylie, W. Hayes
30 December 1975	PMS-405, Washington, D.C.	Discuss Technical problems in which NASA might participate - Dr. D. Finkelman
12 January 1976	NASA/Lewis Research Center, Cleveland, OH	Technical Discussions - D. Connolley, J. Slaby
16 January	NASA/Langley Research Center,	Technical Discussions - Dr. Robert Hess
28 January	NASA/OAST Washington, D.C.	Discuss Progress - Dr. J. Lundholm
2 February	Jet Propulsion Lab. Pasadena, CA	Technical Discussions - Dr. G. Russell
24 February	NASA/OAST Washington, D.C.	Discuss Progress - Dr. J. Lundholm
12 March	NASA/OAST Washington, D.C.	Program and Technical Discussions
8 April	NASA/OAST Washington, D.C.	Program and Technical Discussions and Review Prior to HPL Lewis Meeting - Dr. J. Lundholm
20 & 21 April	NASA/LeRC Cleveland, OH	Participate in NASA HPL meeting - Dr. J. Lundholm, Personnel from NASA/Ames, LeRC, LaRC, and JPL
28 April	WJSA/Huntsville, AL	Scope Work and Review MSPP Task - H. Williams

TRIPS AND CONFERENCES

DATE	FACILITY	PURPOSE OF VISIT AND PERSONNEL CONTACTED
3 May 1976	WJSA/Wakefield, MA	Program Review - Dr. Gerry
4 - 11 May	WJSA/Wakefield, MA	Preparation of Data for Theme Team Meeting for Following Week - Dr. Rather, Mr. H. Williams
11 May	NASA/OAST Washington, D.C.	Participate in Theme Team Meeting and Present Results of WJSA Effort - F.C. Schwenk and Other Theme Team Members
14 May	NASA/OAST Washington, D.C.	Review Results of Theme Team Presentations and Technical Discussions - F.C. Schwenk and Dr. J. Lundholm
19 May	NASA/ARC Mountainview, CA	Review Cleveland Meeting with Ames Personnel - Dr. K. Billman, R. McKenzie et al
25 May	NASA/OAST Washington, D.C.	Technical discussions - F.C. Schwenk, Dr. J. Lundholm, W. Hayes
26 May	NASA/LeRC Cleveland, OH	Technical Discussions and Review of April Meeting - J. Slaby, D. Connolley, et al
11 June	JPL/Pasadena, CA	Review Cleveland Meeting with JPL Personnel - G. Russell et al